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British Monetary Statistics[1]

By Harry G. Johnson

It is now almost exactly seven years since the Government took the first steps towards reviving monetary policy as an instrument of controlling aggregate demand, after an interval of nearly twenty years during which monetary techniques were devoted to the pursuit of one form or another of cheap money. During that seven years, economic policy has come to place increasing weight on monetary policy as the prime instrument for correcting short-run changes in the level of effective demand, while at the same time the operating techniques of monetary management have been adjusted so as to eliminate discrimination in favour of Governmental borrowing in the short-term credit market; and more direct methods of control over various types of borrowing have to a large extent been replaced by indirect monetary restraints of a more traditional type. The appointment of the Radcliffe Committee, it is true, reflected some doubts about the efficacy of monetary policy under modern conditions; but the recent loosening of control over bank lending, in particular with respect to participation in hire purchase finance, marks a continuation of the trend back to reliance on monetary policy of the traditional sort.

In view of the importance of monetary policy in contemporary economic policy, and the modern emphasis on the desirability of adequate statistics relating to economic policy for policy-making, it is remarkable (to say the least) how incomplete and inadequate the published monetary statistics available in this country are. By monetary statistics, here and throughout this paper, I mean the subset of financial statistics pertaining to the quantity of money in existence and its rate of turnover or velocity of circulation. And it seems fair to judge the adequacy or otherwise of statistical information by what is published, since unpublished statistics, however useful they may be to those actually responsible for policy-making, are simply not available to the independent student of the working of the economy—or even to the elected critics of governmental policy.

The explanation of the shortcomings of existing monetary statistics—which will be elaborated on later in this paper—is to be found partly in the fact that they originate from private or once-private sources—the Bank of England and the Committee of London Clearing Bankers; partly in the fact that, though lip-service is often paid to the idea that monetary policy seeks to control the quantity of money, such control is actually conceived of in terms of its effects on the availability of money, as reflected particularly in conditions in the discount market and the lending activities of the Clearing Banks; partly in the fact that the quantity theory of money, which would provide the maximum incentive for the perfection of monetary statistics, has been so completely discredited, in the eyes of most economists, by its Keynesian rival. Finally, there is the inertia of familiarity, by which the reasonably acceptable and understood statistic makes improvement seem not worth the effort.

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[1] A paper read to the Joint Meeting of the Manchester Statistical Society and the Royal Statistical Society, held in Manchester, on 19th–21st September, 1958.

There have in fact been only three important improvements in British monetary statistics since the war:[1] the agreement of December, 1946, by which the London Clearing Banks agreed to draw up their balance-sheets for a common day in the month and to eliminate the " window-dressing " of their cash reserves; the publication of the quarterly analysis of advances made by the members of the British Bankers' Association, beginning in February, 1946;[2] and the publication of the division of Clearing Bank discounts between Treasury Bills and others, beginning with November, 1951.[3] All three of these improvements relate to information about the assets against which deposit money is created, rather than about the liabilities which constitute the supply of deposit money itself;[4] and only one has been introduced since the revival of monetary policy began, and that at a very early stage. On the other hand, the monetary statistics now available are in some respects worse than those which were available before the war, most notably because the Clearing Bank figures now pertain to only one day per month (and a different day in June and December than in other months), instead of being a monthly average of figures for one day in each week; while the bank deposit turnover figures which were published by the Bank of England in the 1930's have disappeared.

The improvement of monetary statistics is evidently one of the subjects which will concern the Radcliffe Committee; a memorandum on the subject has indeed been prepared for the Committee by the National Institute of Economic and Social Research. But how far the Committee's recommendations (whatever they may be) will be translated into practice is doubtful—we are still waiting for some of the improvements the Macmillan Committee recommended—and will depend in part on how strong a feeling there is among users of monetary statistics about the desirability of improvement. The urge to make some propaganda in this direction is, quite frankly, the underlying motivation for this paper. I shall begin by describing briefly the general requirements to which monetary statistics should conform, and criticizing the published British statistics in this light. After that I shall describe two series I have compiled (with the help of a great deal of labour contributed by Miss Alison Martin) from the annual balance-sheets of banks and other financial institutions, in an effort to obtain a more comprehensive measurement of the money supply of the United Kingdom than is commonly used; and I shall finish with a comparison of the results with series drawn up on the conventional concepts, together with some more general remarks.

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[1] See Addendum II.

[2] The " other financial " classification was subdivided into " Hire purchase finance companies " and " Other financial " beginning in February, 1954, at which time certain short-term lendings to stockbrokers not previously incorporated were added; the " Agriculture and fishing " classification was subdivided into its constituents beginning in August, 1955. The total advances of the London Clearing Banks only included in the classification is published for dates from August, 1955, on.

[3] In addition, since June 30th, 1956, the Treasury has published monthly (for the same date as the Clearing Bank returns) the total of outstanding advances to nationalised industries, subdivided between the London Clearing Banks and the Scottish banks. The Companies Act of 1948 must also have brought an improvement in the quality of the Clearing Bank figures.

[4] The standardization of Clearing Bank accounting dates incidentally improved the quality of the deposit statistics.

As already mentioned, I define monetary statistics as statistics of the quantity of money and its rate of turnover. So far as the quantity of money is concerned, there would seem to be three main requirements. The first is that what is being measured is money. This requires a definition of money, in terms susceptible of measurement. The simplest of such definitions, which identifies money with legal tender, unfortunately excludes the larger (and possibly the more variable) part of the means of payment actually in use in a modern economy, namely bank deposits subject to cheque; on the other hand the economist's usual definition in terms of " general acceptability " is too ambiguous to serve as a guide for measurement. For practical statistical purposes, money has to be defined as the sum of two parts: currency (i.e., notes and coin) in circulation, and the liabilities of banking and financial institutions which serve as equivalents of currency. But the notion of an " equivalent " itself requires definition. Two alternative conceptions of equivalence, one narrow and the other broad, are possible. The first would confine the quality of money to liabilities which are direct substitutes for currency, in the sense of being directly usable as media of exchange—that is, liabilities which carry chequing or equivalent facilities. The other would include liabilities which are substitutes for currency at one remove, in the sense of being convertible into media of exchange (currency, or liabilities of the first type) on demand without loss of capital value. This concept is clearly far more extensive than the other; also, since the convertibility of liabilities of this kind is subject to various kinds of restrictions, delays, or sacrifices other than of capital value, there is a problem of defining the boundary between monetary and non-monetary liabilities. Both concepts, in fact, are rather fuzzy around the edges, though the second much more so than the first; and the definition of both for statistical purposes must to some extent be arbitrary—unless one adopts the quite different approach of including in the measurement whatever gives the best results according to some extraneous test (e.g., prediction of some other economic variable). One further point should be made here, which is important later: since institutions with monetary liabilities usually hold some currency, the first part of the money supply, currency in circulation, cannot be measured independently of the second.

The second requirement of good money supply statistics is that, as nearly as possible, all of the money supply in the region or economy should be measured. All institutions (or branches of institutions) in the region with liabilities of a monetary nature should be included, and their monetary liabilities entered in the total; on the other hand, monetary liabilities of one institution held by another, together with currency held as reserves, must be excluded to avoid double counting. Thus the money supply statistics would be built up by consolidating the balance-sheets of all institutions, public and private, with monetary liabilities, eliminating all the contra-items in the process.

The third requirement is that the measurement of all components of the money supply should be made as closely as possible at the same point in time, to avoid double counting and omissions, while the dates of successive measurements should be such as to make the figures both representative and comparable with one another. Money supply figures, like other balance-sheet data, are samples of a changing flow, and should be as good samples as possible.

A fourth requirement, with which I shall not be directly concerned in this paper, is that both the aggregate of monetary liabilities and the assets against which they are created should be broken down into economically significant categories.

With respect to statistics of velocity of circulation, it is self-evident that these should relate to the same monetary items as the money supply figures. Unfortunately, there is no obvious way of estimating the velocity of circulation of currency, though this should be reflected to some extent in (say) the length of time it takes the average banknote to reach a given state of dilapidation. So far as deposits and related forms of money are concerned, the obvious measure is the total of withdrawals in a given period. The inclusion of monetary liabilities not subject to cheque raises a problem of double counting, since, for example, a transfer from a deposit account to a current account to someone else by cheque would appear as two debits; the same difficulty of distinguishing changes in the form of money from transfers of it would appear in an even more acute form in any attempt to measure the velocity of circulation of the total money supply.

Let me now turn to the published statistics, dealing first with velocity figures since they can be dealt with briefly. The only official figures are the Bankers' Clearing House figures for bank clearings, which exclude the important volume of cheques cleared between branches of the same joint-stock bank. The Midland Bank has recently published an index of " Business Turnover " on its current accounts; but (as can be verified by comparing other aspects of their operations) there is no reason for assuming that any one of the Big Five is closely representative of the Clearing Banks as a whole. Some idea of velocity can also be gained from the movement of the Clearing Banks' " Balances with, and cheques in course of collection on, other banks in the United Kingdom and the Irish Republic "; but this figure is not only open to the same objection as the clearing figures, since it excludes transit items, but also has a number of additional defects.

Coming now to statistics of money supply, it is of course a familiar fact that no consolidated statement of the money supply is published.[1] Instead, the authorities publish a do-it-yourself kit consisting of three parts which do not fit together very well and, when fitted together, do not conform to the specifications of what is required.

The first part is the Wednesday Bank of England return, still published in the two-department form laid down in the 1844 Act. From the point of view of money supply statistics, the chief defect of the return is that the item " Bankers' Deposits " (which should cancel out in a consolidated money supply statement) comprises not only deposits belonging to the London Clearing Banks, but also assets of the Scottish and Northern Irish banks and an unspecified group of banks carrying on business mainly in the United Kingdom. As a matter of fact, however, the Bank of England return is not used in the compilation of any index of money supply I am aware of, even though deposits at the Bank of England, other than Bankers' Deposits, are larger than those of two of the Clearing Banks (Coutts and Glyn, Mills) and about as large as those of a third (the National).

The second building-block is the Bank of England's monthly estimate of currency circulation with the public, compiled by taking the total of Bank of England, Scottish, and Northern Irish bank notes outstanding, together with an estimate of coin (other than silver in the Issue Department), and subtracting the notes and coin held by the Bank of England Banking Department, and the Scottish, Northern Irish, and London Clearing Banks. The Bank of England figures are averages of Wednesdays; the same description applies to the Clearing Bank figures, confirming that the Bank of England has better returns available than are published. The Scottish and Northern Irish figures are " averages of Saturdays in the consecutive four-week periods as published in *The London Gazette* or in *The Belfast Gazette* with as close a correspondence as possible to the calendar month "—which may be as much as two weeks ahead of or behind the other figures. Aside from that, the method of construction means that the figures can only with strict consistency be combined with the total deposits of the Bank of England and the Scottish, Northern Irish, and Clearing Banks together, to form an estimate of money supply.

The third piece of construction material is the monthly statement of the London Clearing Banks, the most inadequate of the lot.[2] In coverage it excludes not only the Scottish and Northern Irish banks but also some banks operating in England and Wales with larger assets than some of the banks included; on the other hand, it includes both the Irish Republic business of the National Bank—which in the early 1930's ran as high as 3½ per cent. of the total—and the Eastern business of Lloyds Bank; and (apart from other factors) its scope has been extended from time to time through the absorption of smaller banks by Clearing Banks. Information necessary for accurate money supply statistics is not shown: on the liabilities side, the " Deposit and other accounts " figure includes internal accounts such as profit and loss, dividend, pension fund, and contingency accounts which are not monetary liabilities; on the assets side only one bank (Lloyds) shows cheques in transit between branches, the rest including them with advances to customers. Finally, the dates of the monthly statements cause trouble: for ten months of the year the statements refer to the third Wednesday (except that the National's is for the day before) while in June and December they refer to the last day of the month. Not only does this mean a substantial variation in the actual intervals elapsing between successive " monthly " figures around the half-year ends—on the average five weeks followed by three, though one interval could be as long as 46 days and the other as short as 15—but the half-year-end figures are distorted by an exceptionally heavy volume of cheque payments, and the crediting or debiting of interest and other charges to customers, as well as by the shifting day of the week on which they fall (since there is a regular weekly pattern of movement of funds into and out of the banks).

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[1] See Addendum I.

[2] See Addendum II.

From these materials, one or other of two indices of money supply is usually constructed. One series is Clearing Bank net deposits, obtained by subtracting from the total of current, deposit and other accounts the figure for balances due and cheques in course of collection from other banks in the British Isles. A variant, employed by the Bank of England and the London and Cambridge Economic Service, is to deduct in addition reported Items in Transit; since only one bank reports such items, though another (Martins) did so until 1943, it is very doubtful whether this leads to better rather than worse results. In either case, the procedure makes the questionable assumption that cheques and balances due from non-Clearing Banks are equal to clearing bank liabilities to such banks of the same description.

The other series is obtained by combining Clearing Bank net deposits with estimated currency in circulation with the public, a procedure which, as has already been explained, is logically inconsistent, since it deducts Scottish and Irish bank holdings of currency without adding in their deposits.[1]

The two series so obtained are shown, for the months of December in the years 1930 to 1957 inclusive, in the second and third columns of Table I (p. 8); the first column presents a series derived by adding the net current accounts only of the Clearing Banks to the estimated currency circulation, which corresponds more closely than the other two series to the concept of the money supply as consisting only of media of exchange.[1] These three series are also shown, charted on a semi-logarithmic scale, in the lower part of the chart on page 10.

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[1] Consistency of a sort could be obtained either by adding currency held by the Bank of England and the Scottish and Northern Irish banks, or by adding Bank of England deposits (other than Bankers' Deposits), in each case subtracting the outstanding notes of the Scottish and Northern Irish banks. The second of these would omit from the total Bank of England deposits held by banks other than Clearing Banks.

Before proceeding further, I should like to call your attention to the fact that the three series show rather different movements. Over the whole period, Clearing Bank net deposits increased by 258 per cent., net deposits plus currency by 288 per cent., and net current accounts plus currency by 338 per cent.[2] Behind these differences lies a much greater increase in currency circulation than in net deposits (458 per cent. as compared with 258 per cent.) and in net current accounts than in deposit and other accounts (313 per cent. as compared with 216 per cent.). Over shorter periods, the movements of the three series diverge even more; and over three years (1931–32, 1950–51, and 1956–57) there is a divergence of direction.

The other two columns of Table I contain the results of an attempt to construct more comprehensive measurements of the money supply in this country than are provided by the two major series just discussed. To do so, it was necessary to resort to annual balance-sheets for information on the Scottish and Northern Irish banks and those banks in England and Wales not included in the Clearing Bank return, and to use data on these and other financial institutions less satisfactory than what is now available in order to obtain a long enough statistical run. The results are not particularly satisfactory, because of the dispersion of balance-sheet dates about the focal date of December 31st, the doubtful meaning of some of the figures and the absence of others (especially before the 1948 Companies Act), and the lack of information about some institutions which should be included, such as the branches of overseas banks which carry on business in this country. These considerations, incidentally, explain why (with one exception) the figures have been obtained simply by addition of original balance-sheet figures without any effort to interpolate a figure for December 31st.

The figures in the fourth column of Table I were obtained by adding together the following items: estimated coin outstanding for December; deposits and notes in circulation of the Bank of England for the last Wednesday in December; notes in circulation and current, deposit and other accounts of the London Clearing Banks as shown in the end-year balance-sheet; and the same items (in fact, everything but capital and reserves) for the Scottish banks, the Northern Irish banks, and a variety of other banks, the balance-sheet in each case being assigned to the nearest December 31st. As an indication of relative magnitudes, for 1930, the Clearing Banks accounted for 78.1 per cent., the Scottish banks for 11.3 per cent., the Northern Irish for 2.1 per cent., and the remainder for 8.5 per cent. of the total included liabilities of the four bank groups; in 1957, the proportions were 81.0, 10.4, 1.7 and 6.9 per cent. respectively. From the total of deposits, etc.,

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[1] In all three series, the net deposit figure is obtained from the gross by subtracting balances due and cheques in course of collection on other British banks, but *not* the Lloyds Bank transit item; current accounts for 1930 to 1937 inclusive are estimated from the division of English and Welsh accounts into current and other accounts. The figures before 1936 are not strictly comparable with those from 1937 on, owing to the admission of the District Bank to the Clearing House in that year, which raised Clearing Bank net deposits for that year by £72 millions or just under 3½ per cent. of what they otherwise would have been. It has not seemed worth-while to make detailed adjustments for this.

[2] Excluding the District Bank in 1957, the increases are respectively 244 per cent. for net deposits and 278 per cent. for net deposits plus currency.

TABLE I

BRITISH MONETARY SERIES, 1930 TO 1957

(£ million)

	Net Clearing Bank current accounts plus currency in circulation	Clearing Bank net deposits	Clearing Bank net deposits plus currency in circulation	Net monetary liabilities of the banking system	Net monetary liabilities of financial institutions
1930 ..	1303	1822	2168	2714·5	3297·1
1931 ..	1189	1686	2045	2573·3	3160·0
1932 ..	1317	1933	2295	2811·1	3454·3
1933 ..	1346	1891	2258	2828·1	3531·0
1934 ..	1377	1916	2295	2863·1	3619·2
1935 ..	1494	2030	2431	2981·6	3819·2
1936 ..	1670	2245	2689	3267·7	4186·3
1937 ..	1698	2257	2733	3317·0	4298·5
1938 ..	1663	2188	2661	3232·9	4277·7
1939 ..	1810	2335	2853	3458·1	4567·8
1940 ..	2235	2683	3265	3849·6	5086·9
1941 ..	2719	3183	3880	4628·3	6095·2
1942 ..	3133	3464	4333	5137·5	6895·2
1943 ..	3562	3850	4882	5784·2	7879·2
1944 ..	4069	4369	5569	6500·2	8984·3
1945 ..	4415	4660	6003	7023·7	9890·2
1946 ..	4956	5438	6818	8014·0	11299·8
1947 ..	5035	5682	7007	8325·0	11668·8
1948 ..	5126	5914	7167	8562·3	12064·3
1949 ..	5191	5964	7232	8709·2	12323·4
1950 ..	5283	6100	7389	8777·5	12503·4
1951 ..	5355	6036	7398	8747·8	12330·1
1952 ..	5381	6155	7609	8932·9	12504·1
1953 ..	5528	6370	7895	9261·2	12778·3
1954 ..	5721	6558	8177	9600·2	13212·8
1955 ..	5702	6261	7993	9364·2	12920·0
1956 ..	5758	6305	8144	9469·1	12981·5
1957 ..	5710	6516	8447	9783·5	

NOTES:

Clearing Bank net current accounts and net deposits: December averages to 1938, December 31st from 1939 on; for 1930–37, current accounts estimated from ratio of current accounts to total deposits for England and Wales deposits only. "Net" = after deduction of balances due and cheques in course of collection on other banks in the British Isles.

Currency in circulation: December averages.

Net monetary liabilities of the banking system: see notes to Table II (p. 15).

Net monetary liabilities of financial institutions: see notes to Table III (p. 17).

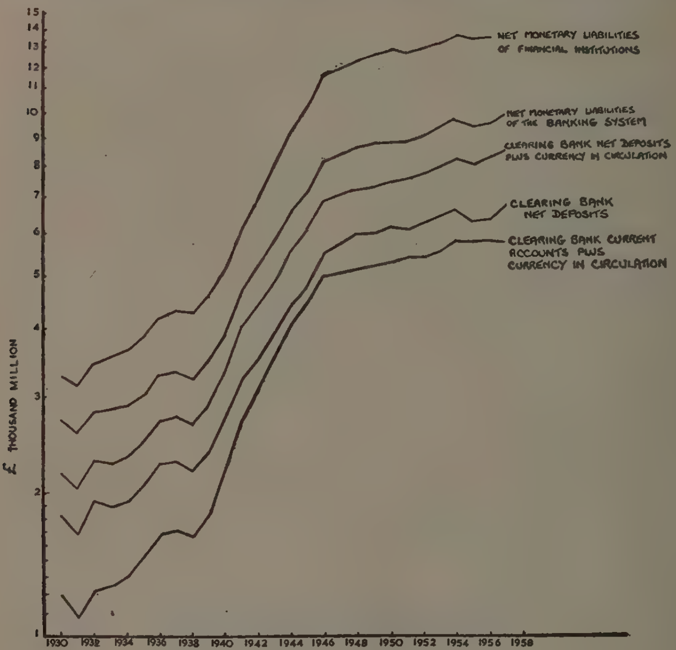
Sources of first three series: *Bank of England Statistical Summary, Monthly Digest of Statistics.*

notes and coin so derived was deducted the total of money held by the banks concerned, consisting of coin held in the Bank of England Banking Department (Issue Department coin is not included in the estimated coin), cash held by the various commercial banks, and balances due and cheques in course of collection for the Clearing, Scottish, and Northern Irish banks (this item is not stated separately by the other banks) to give the final figure shown in the Table.

The series in the fifth column was obtained by adding to the net monetary liabilities of the banking system the monetary liabilities of other financial institutions and deducting their cash holdings. The liabilities included are as follows: the loans and deposits of four discount houses which published balance-sheets throughout the period—these four account for just under sixty per cent. of the loans and deposits of the eleven houses whose balance-sheets are now published; Post Office Savings Bank deposits; Trustee Savings Banks deposits: and Building Society deposits. Since the published annual Building Society figures are taken from balance-sheets with dates ranging from the beginning of February to the next January 31st, the series was centred on the end of December by taking an appropriately weighted average; but the Trustee Savings Bank figures are included as published, though they relate to November 20th. Building Society cash was not separated from other assets until 1948, so that the figures after that year are lower by the amount of Building Society cash than they would be if the series had continued to be compiled on the previous basis; for 1949, the difference was £32.1 million, just over ¼ per cent. of the total, and for 1956 it was £59.0 million, just under ½ per cent. of the total.

The selection of these particular items—deposits of discount houses, Post Office, and Trustee Savings Banks, and Building Societies—as contributing to the community's money supply, and the exclusion of other possible candidates, calls for some explanation. These four are included as being close substitutes for deposit accounts in commercial banks. Post Office and Trustee Savings bank deposits are usually included in National Savings, along with Savings Certificates, Defence Bonds, and Premium Bonds, and it might be argued that the latter should also be included in the aggregate; the chief argument for exclusion is that in each case the rights lost by encashment cannot be regained immediately by re-investment of the funds. There is an even stronger argument for excluding Tax Reserve Certificates, which were once suggested to me by a Lloyds Bank official for inclusion in the money supply. On the other hand, Building Society shares, as distinct from deposits, might perhaps be included, on the grounds that in practice they can usually be encashed on demand; and if the statistics were available one would want to include the deposit and comparable liabilities of other financial institutions, most notably some at least of deposits with hire purchase finance companies.

Let me turn now from the method of compilation to the results: for ease of comparison, the five series are graphed on a semi-logarithmic chart, which has the advantage that the same vertical distance anywhere on the chart represents the same proportionate relationship. The first impression that one obtains from this chart is of a very close similarity of movement of all five series; this after all is only to be expected, given that the higher series are compiled by adding to the lower ones, and given also the strong common forces which have been operating on all financial institutions and the historically short period which the series cover. But there are some significant and interesting differences, both in the movements of the series over the whole period and in their movement over shorter stretches of time.



BRITISH MONETARY SERIES, 1930-1957

In the first place, comparison of the two more comprehensive series shows that net monetary liabilities of financial institutions have grown more rapidly than net monetary liabilities of the banking system—the respective increases between 1930 and 1956 are 294 per cent. and 249 per cent. Moreover, contrary to what one might be led to expect by some arguments of the past few years, the more rapid growth of the contribution to the money supply of non-banking institutions over the period is entirely accounted for by more rapid growth before 1951: since 1950, net monetary liabilities of all financial institutions have grown less rapidly than the money supplied by the banking system. In other words, the proportion of the money supply coming from non-banking-system sources increased during the cheap money period (from under 19 per cent. in 1931–32 to 30 per cent. in 1950) and has been decreasing under the influence of the credit squeeze (to about 27½ per cent in 1956). It would be tempting to take this as refuting the argument that a credit squeeze merely induces people to find substitutes for customary forms of money; but this would be an invalid inference, since the figures cannot claim to include all substitutes for money.

Comparison of the two new series with the more familiar ones shows, first, that the growth of the net deposits plus currency series (276 per cent. between 1930 and 1956) has been rather greater than that of aggregate monetary liabilities of the banking system but less than that of the monetary liabilities of all financial institutions. Oddly enough, Clearing Bank net deposits have grown at almost exactly the same rate as money supplied by the banking system (an increase of 246 per cent. as compared with 249 per cent.) and this close relationship also seems to hold fairly well for shorter-run movements. Current accounts plus currency, the media-of-exchange series, has grown faster over the period than any of the others; its ratio to the banking system monetary liabilities series rose from 46 per cent. for 1931 to a peak of 63 per cent. for 1945, and has since varied around the level of 60 per cent.

Time prevents a detailed comparison of shorter-run movements, and permits merely a mention of one interesting point. This relates to the experience of cheap money in the 1930's: while the two customary series, net deposits and net deposits plus currency, give the impression of a burst of monetary expansion in 1932, followed by a partial contraction, the more comprehensive series, especially the larger, give the impression of a much steadier expansion of the money supply from 1931 to 1938, an impression which is also supported by the series of current accounts plus currency. The contrast suggests the fascinating problem of how monetary expansion propagates itself, which I cannot pursue here. It also illustrates the general point that the detailed movements of the various series can differ in economically significant ways.

For the explanation of movements in the money supply, as distinct from measurement of such movements, it is necessary to have statistical information on the assets against which monetary liabilities are created. (Such information is, of course, useful in itself, for other purposes than the explanation of changes in money supply.) For the two sets of measurements I have made, this information is presented in consolidated balance-sheet form, in Tables II and III (pp. 14 & 16). I regret that time prevents me from elaborating on either the construction and content of the figures, or the interesting inferences that can be drawn from them. One point is, however, worth mentioning, and that is the great change which has occurred in the proportions in which the money supply is backed by private assets and public debt respectively. This is reflected, for the banking system, in the fall in bank advances from just over

40 per cent. of total assets in 1930, to 24–25 per cent. in recent years; the shift from private to public debt is probably even greater, since the proportion of private debt in the other items, especially call money and discounts, is also probably lower than it used to be. For the aggregate of financial system monetary liabilities, the proportion of advances and loans and balances due on mortgages has fallen from 39.6 per cent. to 28.5 per cent. over the period; it is also interesting that balances due on mortgages now amount to about three-quarters as much as advances and loans.

In concluding this paper, I should like to return to my initial theme, the inadequacy of existing British monetary statistics. The new series I have compiled, unsatisfactory as they are, do I think demonstrate that while existing series are probably adequate as indicators of broad movements of the money supply, they are not necessarily good indicators of short-run movements; while even the broad movements of measurements based on different definitions of money supply do differ in significant ways. In addition, of course, there are some purposes for which one wants the total money supply, and not merely an indicator of its movements. (As an example of the difference the use of different series may make, on the basis of the net deposits plus currency series the gold and dollar reserves of the country have fallen from the end of 1937 peak of 31 per cent. of the money supply to 9.3 per cent. at the end of 1956; on the basis of the financial institution net monetary liabilities the decrease has been from 19.2 per cent to 5.9 per cent.). If monetary policy is to become increasingly the central instrument of economic policy, there seems to me to be a strong case for improving the statistical material available for monetary analysis.

University of Manchester.

ADDENDUM I

During the discussion of this paper, Sir Harry Campion pointed out that a compilation of the money supply had been carried out by the Central Statistical Office with the assistance of the Bank of England, the results of which were published in the February, 1958, issue of *Economic Trends*. This publication, which is not listed in the index of the issue, takes the form of three charts (pp. iv to vi) unaccompanied by either an explanatory text or a table of the original series. The original series apparently consists of a single figure for each year from 1948 to 1957;. it is not stated whether the constituents of this figure are single observations or (as would seem more likely) annual averages of monthly figures.[1] The Notes describe money supply as:

> " Total of (1) deposits at London Clearing Banks and Scottish Banks (less collections and identifiable items in transit);
> (2) deposits at the Bank of England (other than ' Bankers ');
> (3) currency in circulation with the public."

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[1] In subsequent correspondence, Sir Harry Campion has confirmed that these constituents were averages of monthly figures for the London Clearing Banks, Scottish Banks, and Currency Circulation with the Public, and averages of weekly figures for the Bank of England. He has also pointed out that the figures on which the charts were based were published in the *Economic Survey for 1958* (Cmnd. 394) in Table 15 of Appendix A, for the years 1953–57.

This measurement of the money supply is inconsistent, since it omits the monetary liabilities of the Northern Irish banks and of non-Clearing Banks operating in England and Wales, while deducting from the total the deposits held by these banks at the Bank of England; in addition, if the " currency in circulation " figures are the standard series, currency held by the Northern Irish banks is excluded from the total, though their liabilities are not included.

ADDENDUM II

Beginning with October, 1958, the " Monthly Statement of Balances of London Clearing Banks " has contained more information than was previously published, the new data being presented in the not altogether happy form of footnotes to the main Statement.[1]

The footnotes show:

(1) the division of " Coin, Bank Notes and Balances with the Bank of England " into " Cash in Hand " and " Balance at Bank of England ";
(2) the division of " Money at Call and Short Notice " between " to the Money Market " and " Other ";
(3) the division of " Other Bills Discounted " between " Other U.K. Bills Discounted " and " Other ";
(4) the division of " Investments at Book Value " between " Securities of, or guaranteed by the British Government " and " Other Investments ";
(5) the division of " Liabilities of Customers for Acceptances, Endorsements, etc.", between " Acceptance and Confirmed Credits " and " Other Engagements ";
(6) the amount of " Items in Transit ", of which a small part (presumably that for Lloyds Bank) is included in " Balances with and Cheques in course of collection on other Banks in U.K. and Republic of Ireland " and the remainder in " Advances to Customers and other Accounts " in the Statement.

Of these improvements, (1), (2) and (6) should be particularly useful for the compilation of money supply statistics. But a preliminary exercise with the October figures revealed a puzzle: the Banks' stated " Balance at Bank of England " (£250·9 million) was higher than the amount of " Bankers Deposits " reported by the Bank of England for the same date, (£246·8 million) even though the latter figure relates to more banks than the Clearing Banks' return. The explanation is associated with the fact referred to in the paper, that Lloyds' statement includes the Bank's eastern business—Lloyds' balances with Eastern Reserve banks were counted as balances at the Bank of England. For November these balances were included in cash in hand; but the stated " Balance at Bank of England " (£238·5 million) still exceeded " Bankers Deposits " (£236·7 million). The explanation is presumably some sort of " float ", concealed elsewhere in the Bank's accounts—the National's habit of making up on Tuesdays can hardly explain it, since its total cash was only £7·7 million.

Despite the improvements mentioned, the Clearing Bank figures still leave much to be desired, particularly on the liabilities side. Even now there is no clean figure for deposit liabilities to the public. But the ice has at least begun to crack, and further improvements may well be in store, especially as the Radcliffe Committee has yet to produce its Report.

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[1] Whereas many publications reproduce the details or the totals of the Statement, the footnotes are generally left to the vagaries of editorial comment; and the original Statements are hard to come by, once time has elapsed.

TABLE II

CONSOLIDATED BALANCE-SHEET OF THE BANKING SYSTEM, 1930–57
(£ million)

	LIABILITIES					ASSETS							
	Gross Deposits, Notes and Coin	– Cash and Balances	= Net Monetary Liabilities	Net Capital	Total net Liabilities	Estimated Coin	Issue Department Gold and Silver	Call Money	Discounts	Bank Advances	Investments	Bank Premises etc.	Total
1930 ..	3163·6	449·1	2714·5	196·6	2911·1	69·0	152·0	174·8	402·7	1188·9	865·9	57·8	2911·1
1931 ..	2969·3	395·9	2573·3	187·8	2761·1	67·0	124·8	150·3	291·8	1149·6	918·7	59·0	2761·1
1932 ..	3229·2	418·1	2811·1	190·3	3001·4	67·0	123·5	168·6	437·7	994·3	1151·0	59·3	3001·4
1933 ..	3277·1	449·0	2828·1	190·6	3018·8	67·0	194·3	161·8	331·2	956·6	1248·2	59·6	3018·8
1934 ..	3299·3	436·2	2863·1	194·0	3057·1	69·0	194·7	198·9	279·7	970·4	1285·2	59·2	3057·1
1935 ..	3440·5	458·9	2981·6	196·7	3178·4	69·0	201·3	193·7	331·9	997·6	1326·2	58·6	3178·4
1936 ..	3763·3	495·6	3267·7	200·4	3468·1	72·0	313·7	239·2	324·2	1078·9	1382·6	57·5	3468·1
1937 ..	3806·9	489·9	3317·0	203·9	3520·9	76·0	326·4	197·5	310·4	1189·5	1363·4	57·7	3520·9
1938 ..	3723·5	490·6	3232·9	205·6	3438·5	78·0	326·7	183·5	280·1	1190·0	1320·6	59·7	3438·5
1939 ..	3966·6	508·5	3458·1	203·5	3661·5	83·0	0·9	207·3	359·8	1226·0	1722·9	61·7	3661·5
1940 ..	4423·3	573·6	3849·6	205·9	4055·5	89·0	0·3	192·8	621·9	1111·5	1978·4	61·7	4055·5
1941 ..	5302·7	674·4	4628·3	207·4	4835·6	96·0	0·3	178·0	991·1	1000·7	2509·8	59·9	4835·6
1942 ..	5877·6	740·1	5137·5	208·7	5346·2	105·0	1·0	184·7	1165·3	968·1	2863·5	58·6	5346·2
1943 ..	6583·6	799·4	5784·2	212·7	5996·9	116·0	0·3	197·7	1519·2	935·4	3171·1	57·1	5996·9
1944 ..	7380·7	880·4	6500·2	216·7	6716·9	124·0	0·3	256·2	1921·4	947·4	3411·1	56·5	6716·9
1945 ..	7968·0	944·3	7023·7	219·7	7243·4	132·0	0·3	312·2	2007·4	1010·4	3725·4	55·8	7243·4
1946 ..	8996·3	982·3	8014·0	224·2	8238·3	142·0	0·3	501·8	2287·1	1204·5	4047·1	55·5	8238·3
1947 ..	9399·4	1074·4	8325·0	226·0	8550·9	147·0	0·3	554·6	2189·1	1479·9	4123·5	56·5	8550·9
1948 ..	9638·9	1076·6	8562·3	233·4	8795·8	169·0	0·3	553·4	2247·3	1693·4	4070·1	62·3	8795·8
1949 ..	9760·5	1051·3	8709·2	229·2	8938·4	170·0	0·4	640·5	1978·3	1887·1	4197·4	64·6	8938·4
1950 ..	9866·8	1089·3	8777·5	229·7	9007·3	163·0	4·4	662·6	1950·1	2053·3	4106·7	67·2	9007·3
1951 ..	9873·7	1125·9	8747·8	228·5	8976·3	163·0	2·9	670·7	1138·1	2411·4	4519·9	70·3	8976·3
1952 ..	10141·2	1208·3	8932·9	239·4	9172·4	163·0	2·9	623·9	1310·3	2198·4	4801·0	72·8	9172·4
1953 ..	10486·0	1224·8	9261·2	252·3	9513·5	165·0	2·9	590·6	1466·1	2161·9	5052·9	74·1	9513·5
1954 ..	10906·1	1305·9	9600·2	268·0	9868·2	161·0	3·4	589·9	1375·3	2391·6	5270·2	76·9	9868·2
1955 ..	10626·1	1261·9	9364·2	275·1	9639·3	166·0	3·4	599·3	1495·4	2349·6	4942·2	83·4	9639·3
1956 ..	10760·6	1291·5	9469·1	280·4	9749·5	173·0	3·4	592·9	1483·5	2423·2	4982·5	90·9	9749·5
1957 ..	11179·6	1396·1	9783·5	290·3	10073·8	180·0	3·4	614·6	1624·5	2404·1	5148·7	98·5	10073·8

NOTES:

Balance-sheets included and dates:

Estimated Coin Outstanding (outside the Issue Department of the Bank of England): December averages.

Bank of England: last Wednesday in December.

Eleven Clearing Banks: December 31st.

Scottish banks:

Bank of Scotland, February 28th of following year; Union Bank of Scotland, following April 2nd, 1930–52, following February 28th, 1953, thereafter amalgamated with Bank of Scotland; British Linen Bank, following January 15th, 1930–46, September 30th thereafter; Clydesdale Bank and North of Scotland Bank (amalgamated with Clydesdale, 1950), December 31st; Commercial Bank of Scotland, October 31st; National Bank of Scotland, November 1st; Royal Bank of Scotland, second Saturday in October, 1930–53, thereafter December 31st.

Northern Irish banks:

Belfast Banking Company, December 31st; Northern Bank, August 31st, 1930, thereafter December 31st; Ulster Bank, November 30th, 1930, thereafter December 31st.

Other banks:

Baker Duncombe, September 30th, 1930–33 (liquidated October, 1933); Baring Brothers, 31st December; Blydenstein, December 31st, 1930–33, September 30th, 1934–44, following March 31st, 1945 on; British Mutual, December 31st, 1930–49, no balance-sheet 1950 (absorbed by Martins, January, 1951); Erlangers, 31st December; Grace Brothers, December 31st, 1930–32, June 30th, 1933–36 (liquidated thereafter); Grindlays, October 31st, 1930–40, April 30th, 1941, October 31st, 1942–49, December 31st, 1950 on; Hambros, March 31st of following year; Hoare, July 5th; Isle of Man, December 31st; Japhet, 31st December; Manchester and County, December 31st, 1930–34 (amalgamated with District, July, 1935); Reliance, March 31st of following year; Union Bank of Manchester, December 31st, 1930–39 (absorbed by Barclays, 1940); Yorkshire Penny, December 31st; Birmingham Municipal, March 31st of following year; Co-operative Wholesale Society, second Saturday of following January, except 1930 and 1932, following July; Scottish Co-operative Wholesale Society, third Saturday of following January, 1948 (commencement) on.

Gross Deposits, Notes and Coin: estimated coin in circulation; Bank of England notes and deposits; commercial bank notes and deposits.

Cash and Balances: Banking Department Notes and Coin; coin, notes and deposits at Bank of England held by commercial banks; balances due and cheques in course of collection from other banks (including balances in account with subsidiaries, but excluding declared balances with banks abroad, which are included in advances); money at call and short notice included with cash by British Linen, 1930–40, and all Other Banks except Blydenstein, British Mutual 1946–48, Erlangers, Grindlays 1948–57, Isle of Man 1948–54, Japhet.

Net Capital: capital and reserves, less investments in subsidiaries where identified.

Call Money: Clearing and Scottish banks, except British Linen 1930–40; Blydenstein, Erlangers, Japhet, British Mutual 1946–48; Grindlays 1948–57, Isle of Man 1948–54; includes British Treasury bills held by National, 1930–53.

Discounts: Bank of England "Discounts and Advances"; Clearing Banks, except British Treasury Bills for Glyn, Mills 1930–34 and National 1930–53; Scottish banks, except British Linen discounts 1930–40; Belfast, Ulster 1944–57; Baring Bros., Blydenstein, British Mutual 1947–48; Erlangers, Grindlays 1948–57, Hambros, Hoare 1956, Isle of Man 1956–57, Japhet, Manchester and County, Yorkshire Penny Bank, Union Bank of Manchester.

Advances: includes discounts for British Linen 1930–40; Northern, Ulster 1930–43; Baker Duncombe, Grace Bros., Grindlays 1930–47, Hoare, Isle of Man; goods in stock, for two C.W.S. banks, balances with banks abroad for Lloyds, Martins, National Bank of Scotland 1948–57, Blydenstein.

Investments: includes Treasury Bills for Glyn, Mills 1930–34; Japhet; possibly others.

Sources for balance-sheets: *Bankers' Almanac and Year Book; Economist Banking Supplement; Bankers' Magazine; The Times*, and directly from various banks included.

TABLE III

CONSOLIDATED BALANCE-SHEET OF FINANCIAL INSTITUTIONS, 1930–56

(£ million)

	LIABILITIES							ASSETS								
				Net capital												
	Gross Deposits, Notes and Coin	– Cash and Balances	= Net Monetary Liabilities	Building Societies	Other Institutions	Total	Total Net Liabilities	Estimated Coin	Issue Department Gold & Silver	Call Money	Discounts	Advances and Loans	Balances due on Mortgages	Investments	Bank Premises etc.	Total Assets
1930 ..	3828·0	530·9	3297·1	339·4	210·1	549·5	3846·6	69·0	152·0	174·8	497·9	1190·8	336·4	1351·9	73·7	3846·5
1931 ..	3631·6	471·6	3160·0	380·5	201·6	582·1	3742·1	67·0	124·8	150·3	378·7	1150·8	373·1	1419·3	78·1	3742·2
1932 ..	3953·3	499·0	3454·3	410·4	204·7	615·1	4069·4	67·0	123·5	168·6	535·6	997·2	404·5	1691·9	81·0	4069·2
1933 ..	4065·4	534·4	3531·0	434·2	205·8	640·0	4171·0	67·0	194·3	161·8	427·7	958·5	447·7	1833·0	81·1	4171·1
1934 ..	4147·5	528·3	3619·2	463·6	209·9	673·5	4292·7	69·0	194·7	198·9	359·9	973·8	500·7	1915·7	79·9	4292·7
1935 ..	4379·5	560·3	3819·2	494·8	213·3	708·1	4527·3	69·0	201·3	193·7	418·1	1003·0	555·8	2007·8	78·4	4527·2
1936 ..	4796·7	610·4	4186·3	533·5	218·3	751·8	4938·1	72·0	313·7	239·2	416·7	1083·6	609·4	2125·7	78·0	4938·2
1937 ..	4905·1	606·6	4298·5	571·3	222·4	793·7	5092·2	76·0	326·4	197·5	406·9	1193·2	659·5	2153·6	79·0	5092·3
1938 ..	4880·5	602·8	4277·7	595·9	224·7	820·6	5098·3	78·0	326·7	183·5	392·3	1194·8	695·4	2146·2	81·5	5098·2
1939 ..	5212·0	644·2	4567·8	600·6	222·8	823·4	5391·2	83·0	0·9	207·3	495·9	1228·5	692·9	2596·5	86·2	5391·3
1940 ..	5821·2	734·3	5086·9	596·4	225·8	822·2	5909·1	89·0	0·3	192·8	735·9	1113·0	662·3	3025·5	90·4	5909·1
1941 ..	6963·2	868·0	6095·2	596·3	227·8	824·1	6919·3	96·0	0·3	178·0	1115·5	1002·0	627·7	3806·0	93·9	6919·2
1942 ..	7850·6	955·4	6895·2	605·3	230·1	835·4	7730·6	105·0	1·0	184·7	1343·2	969·4	593·7	4437·3	96·5	7730·8
1943 ..	8918·2	1039·0	7879·2	623·9	236·0	859·9	8739·1	116·0	0·3	197·8	1721·1	936·7	569·7	5102·8	94·8	8739·0
1944 ..	10097·8	1113·5	8984·3	645·9	241·2	887·1	9871·4	124·0	0·3	256·2	2167·3	949·0	567·1	5713·5	94·1	9871·4
1945 ..	11088·3	1198·1	9890·2	678·1	245·8	923·9	10814·1	132·0	0·3	312·2	2273·8	1012·6	604·1	6386·0	93·2	10814·2
1946 ..	12610·1	1310·3	11299·8	729·4	257·1	986·5	12286·3	142·0	0·3	501·8	2660·6	1206·8	683·0	6995·8	96·0	12286·2
1947 ..	13076·6	1407·8	11668·8	793·3	262·0	1055·3	12724·1	147·0	0·3	554·6	2562·7	1483·1	780·9	7095·5	100·0	12723·9
1948 ..	13524·5	1460·2	12064·3	872·9	271·1	1144·0	13208·3	169·0	0·3	553·4	2609·5	1697·5	890·4	7196·5	91·8	13208·3
1949 ..	13742·2	1418·8	12323·4	973·4	268·9	1242·3	13565·7	170·0	0·4	640·5	2434·8	1889·8	1002·3	7347·9	80·1	13565·6
1950 ..	13999·2	1495·8	12503·4	1078·0	270·7	1348·7	13852·1	163·0	4·4	662·6	2439·3	2055·7	1104·9	7337·9	84·5	13852·2
1951 ..	13903·7	1573·6	12330·1	1191·8	270·2	1462·0	13792·1	163·0	2·9	670·7	1532·9	2415·5	1206·3	7712·9	88·0	13792·0
1952 ..	14179·4	1675·3	12504·1	1336·0	281·6	1617·6	14121·7	163·0	2·9	623·9	1761·5	2203·5	1324·1	7952·5	90·4	14121·7
1953 ..	14502·1	1723·8	12778·3	1515·0	295·7	1810·7	14589·0	165·0	2·9	590·6	1845·4	2177·8	1477·5	8236·8	93·0	14589·0
1954 ..	15095·3	1882·5	13212·8	1718·1	313·5	2031·6	15244·4	161·0	3·4	589·9	1794·5	2409·3	1655·3	8532·6	98·5	15244·5
1955 ..	14732·9	1812·9	12920·0	1913·7	321·3	2235·0	15155·0	166·0	3·4	599·3	1917·6	2358·4	1811·3	8192·8	106·1	15155·0
1956 ..	14834·2	1852·7	12981·5	2093·7	327·1	2420·8	15402·3	173·0	3·4	592·9	1865·8	2438·9	1947·8	8263·7	116·9	15402·4

NOTES:

Balance-sheets included, and dates:

All those in Table II.

Discount Houses: Alexanders, December 31st; Gillett Bros., following January 31st; National, December 31st; Union, December 31st.

Trustee Savings Banks: Ordinary and Special Investment Departments, November 20th;

Post Office Savings Banks, December 31st.

Building Societies: $\frac{13}{24}$ of current year total plus $\frac{11}{24}$ of total for following year, since " December " totals actually run from February 1st to following January 31st.

Discount Houses: liabilities for " bills rediscounted " and " rebate on bills discounted " subtracted from " bills discounted " on assets side to give a net figure.

Trustee Savings Banks: " Net Capital " and " Premises " include a variety of other accounts.

Building Societies: " Cash " not shown separately until 1949. " Premises " includes " Other assets "; for 1930–34, this item was estimated by dividing the investment total (in which it was included) in the average proportion shown for 1936–38 (which was fairly constant).

Building Society share capital is stated separately for the convenience of anyone desiring to treat it as monetary. Of the non-banking institutions, only the Discount Houses contribute to " Discounts " and " Advances and Loans ".

Sources for non-banking institution balance-sheets: *Bankers' Almanac and Year Book; Building Society Year Book; Post Office Savings Banks Accounts; Trustee Savings Bank Year Book; Reports of the Inspection Committee of Trustee Savings Banks.*

Builders' Wage-rates, Prices and Population: Some Further Evidence

By E. H. Phelps Brown and Sheila V. Hopkins[1]

I

In two previous papers[2] we gave estimates indicating that the basketful of consumables which the building craftsman could buy with his day's pay contracted greatly in the course of the sixteenth century in Southern England, France and Alsace, until in all three it was less than half as big as it had been. Our estimates also indicated that this had come about despite the builders' money wage-rate having risen in much the same proportion as the prices of industrial products: the prices of foodstuffs had risen very much more, so that the quantity of foodstuffs which the industrial worker could obtain in exchange for a unit of his own produce was greatly reduced. We suggested that this wide movement of the terms of trade between workshop and farm, itself also common to all three regions, might have a common cause in the increase of population: if the agriculture of the time could not absorb many of the extra hands, most of them must work in industry if they were to work at all, and so the output of industry would be increased relatively to that of agriculture, and industrial products and the labour that made them would alike be cheapened in terms of food. We can now add estimates which will show whether anything of the same kind seems to have happened in Münster, Augsburg, Vienna and Valencia.

In all four, the wages and prices alike come from the books kept mainly by religious foundations and hospitals. Münster provides some accounts from the municipality, but more from the cathedral, five monasteries, and ten local offices of the bishop's administration; there are many more quotations from the country than the city, but all are from within forty square miles. All the figures for Augsburg come from its hospital. It is the city hospital too, that yields a large part of our figures for Vienna, but many come also from the Klosterneuburg monastery seven miles or so away—from the accounts kept in its own offices and by the priests of its parishes. In Valencia, the accounts of the cathedral provide the costs of builders' work, those of the Hospital dels Inocents the prices of consumables; from 1501 onwards the municipal accounts yield wages. Many of the various foundations owned their own land, and recorded not only the prices at which they bought supplies for their residents, but those at which they sold farm produce, or accepted it in payment of rent or tithe.

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[1] It was only through the generous guidance to the sources given us by Dr. D. C. Coleman that this study was made possible; and we are indebted to him too for reading and discussing this paper in draft. We also thank Mrs. Meyrick Browne, who carried through the whole study of Augsburg, and made the index of French land prices; Dr. Dipak Mazumdar, who made the first survey of the Spanish materials; Mrs. Faye Bayley, who took part in the work on Vienna, and carried through the computations for Valencia; and Miss Sheila Chantler, who drew the Figures. We are grateful to Professors E. M. Carus-Wilson and F. J. Fisher for their kindness in reading our paper in draft.

[2] "Seven Centuries of the Prices of Consumables, compared with Builders' Wage-rates", *Economica*, November, 1956; "Wage-rates and Prices: Evidence for Population Pressure in the Sixteenth Century", *Economica*, November, 1957.

We have set out in the Appendix the details of our own secondary sources, and of our methods, but would record here how much we owe to the immense labours of the original searchers and gatherers—Georg Wiebe, Moritz Elsas, Karl Pribram, and Earl Hamilton.

The pattern of the building craftsman's wage-rates is much the same in most of these records as in those we have drawn on before, and again we have struck no averages (save that these were all we had for Münster) but tried instead to pick out a continuous line of rates which were representative in so far as they were recurrent. But some features of the present records are unusual. Generally, a graph of wages can be immediately distinguished from one of prices by its lack of sustained falls and falling trends, but here there are two clear cases of a cut. In Valencia rates generally were reduced substantially in the 1460's: the master carpenter came down from 60 diners a day to 51, the labourer—though this rather later—from 33 to 24. There they stayed till about 1488, when they went back to what they had been. There was little change in prices before or during the cut. In Vienna in 1667 the wage-rates stood where they had been, the one for the past 26 and the other for the past 40 years, the journeyman at 22 kroner a day and the labourer at 15; but by 1670, the one was down to 19½ kroner, the other to 12, and there they stayed through 1682. It turned out that at the new level the journeyman was soon to be as well off as before, for prices went down too; but not the labourer, who had been cut deeper; and in any case the cuts were made just when six successive years of higher prices had reduced the purchasing power of the going rate. Valencian wages also compose one unusual pattern. Most of the rates recorded there fall into the familiar pattern of tramlines, the same two rates cropping up for years together; but the master carpenter's rate, though on tramlines from 1465 onwards, in 1419–64 looks much more like a price series, with quotations at fine intervals, and short-period fluctuations about the majestic arc of a long-period rise and fall. It does seem that there was something unusual in the terms of engagement of the master carpenters by the municipality and cathedral of Valencia at this time; but on further scrutiny we thought the unusual element lay in the variety and fineness of the variations, rather than in any gradual rise and fall of a central value, which itself could quite possibly have remained unchanged, as the rates of the other craftsmen evidently did then.

The commodities whose prices we combined appear in Table I. The list is very like that for the three regions we studied before. The table notices some of the biggest gaps, but there were many others, and we had to use our judgment as before how best to bridge each gap where it occurred.

TABLE I

COMMODITIES ENTERING INTO COMPOSITE UNIT OF CONSUMABLES; WITH OTHER PRODUCTS USED IN INDEX OF PRICES OF INDUSTRIAL PRODUCTS

	MÜNSTER	AUGSBURG	VIENNA	VALENCIA
1. FARINACEOUS	Barley, rye, wheat, oats, peas	Barley, rye, oats, peas, spelt; from 1674 only, wheat	(Not before 1486) barley, "Korn", wheat	Wheat; from 1504 and 1512 only, rice, carob beans
Weight	20	20	20	30
2. MEAT AND FISH	Fat oxen, calves, pigs, fat sheep; herrings, stockfish	Beef, veal, suet, dripping; carp	Hungarian oxen; calves, whole sheep, half or quarter sheep; herrings, sturgeon	Beef (not after 1446); mutton, hens (except 1449 – 1502); lard (from 1502); sardines, hake (not after 1495); tuna (except 1451 – 1503); dried cod (from 1555)
Weight	25	25	37½	25
3. BUTTER & CHEESE	Butter, cheese	Milk; from 1580 only, Dutch cheese	None	Cheese (except 1551–1600)
Weight	12½	12½	—	12½
4. DRINK	Wine, malt, five sorts of beer	From 1641, brown beer	Five sorts of beer; heuriger wine	Wine
Weight	22½	22½	22½	12½
5. FUEL AND LIGHT	Charcoal; tallow, tallow candles, wax	Tallow, linseed oil	Soft firewood, charcoal; tallow candles, raw tallow, olive oil	Firewood; olive oil (not after 1500); charcoal (from 1512)
Weight	7½	7½	7½	7½
6. TEXTILES	Four sorts of linen, five sorts of cloth	Several qualities of twill	Hemp, thread, coarse woollen cloth	Flax; linen, bedticks (from 1552)
Weight	12½	12½	12½	12½
Total	100	100	100	100
ADDITIONAL INDUSTRIAL PRODUCTS	Salt, bricks, tiles, nails, paper, men's shoes	Bavarian salt; lime, bricks; writing paper	Bricks, tiles, shingles, lime, pitch, three sorts of planks, laths; three sorts of nails	Plaster of Paris, lime, bricks; nails, large (not after 1500)

So with some piecing and patching we arrived at an index of the price of a composite unit of consumables in each city. When we confront these indexes with those of the craftsman's wage-rate, what do we find?

The outstanding finding is that in all four places the basketful a day's pay would buy shrank drastically during the 16th century, just as it did in the three regions we studied before. Indexes of the size of the basketful are displayed in Fig. 1. In Fig. 2 they may be compared with those for the other three regions: the base of those three has been changed here to 1521–30, which we chose for the present four because it is the earliest decade that brings Vienna in; also we have smoothed the outlines by taking decennial averages, save for France, which runs in quarter-centuries. The extent of agreement springs to the eye. It is instanced numerically in Table II.

TABLE II

INDEXES OF THE EQUIVALENT OF THE WAGE-RATE OF A BUILDING CRAFTSMAN EXPRESSED IN A COMPOSITE PHYSICAL UNIT OF CONSUMABLES

Base 1521–30=100

	1476–1500	1521–30	1551–60	1591–1600
S. England ..	146	100	(72)*	65
France	131	100	—	52†
Alsace	118	100	68	39
Münster ..	143	100	70	—
Augsburg ..	—	100	78	60
Vienna ..	—	100	69	48
Valencia ..	119	100	67	59

* 1552 only. † 1576–1600.

That Valencia should show so great a fall—a halving of the basketful the craftsman had been buying at the end of the fifteenth century—was unexpected, when Earl Hamilton had found[1] that real wages in Spain in the last decade of the sixteenth century were only about 10 per cent. lower than they had been in the first, and that they then rose again " by leaps and bounds " until in 1611–20 they were higher than ever before since 1500. Hamilton's price index, though unweighted, differs little from ours, but his money wage index[2] differs greatly—in the course of the sixteenth century ours rises by a half, his threefold. This extraordinary disparity may arise from our following only carpenters, stonecutters and masons in Valencia, whereas he combines " wages covering from about 50 to almost 100 grades of labour ",[3] not necessarily the same in each of the three half-centuries from 1501 to 1650, and not in Valencia only but in Andalusia, New Castile and Old Castile as well. " The greatest defect . . . consists of the abnormally high

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[1] Earl J. Hamilton, *American Treasure and the Price Revolution in Spain, 1501–1650* (Harvard, 1934), pp. 277–282.

[2] *Op. cit.*, p. 271, Table 27.

[3] *Op. cit.*, p. 270.

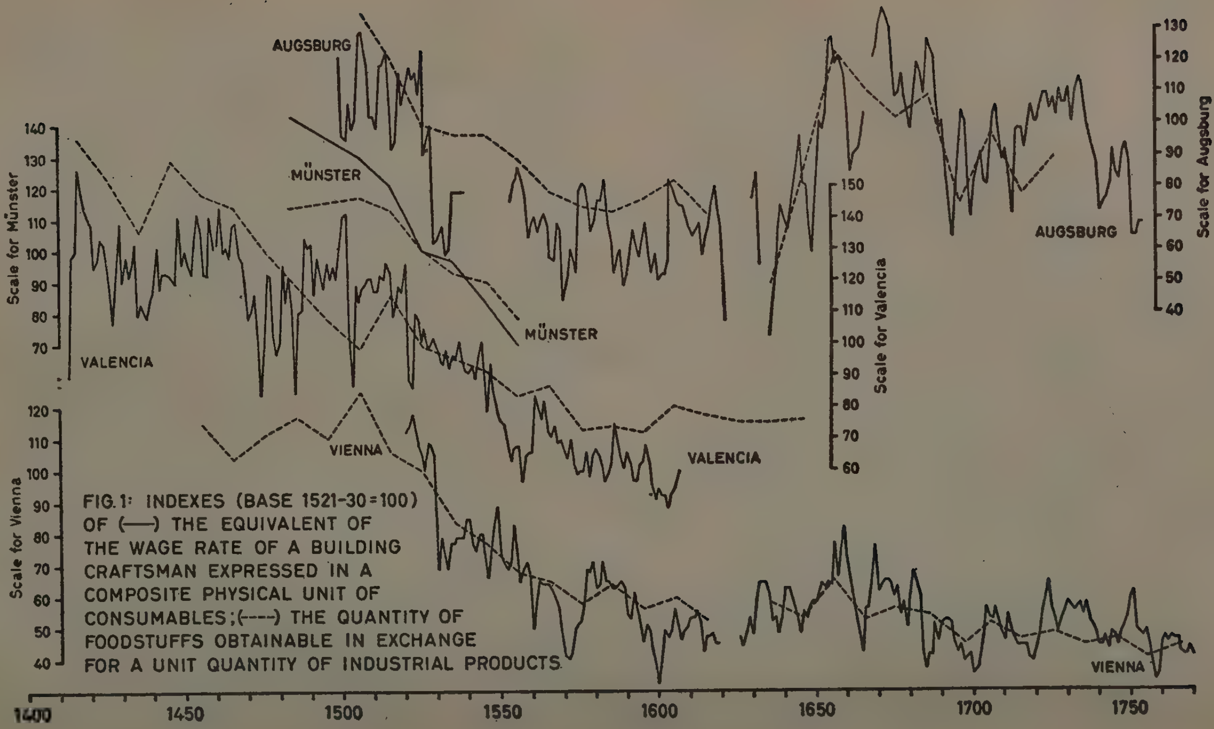

FIG. 1: INDEXES (BASE 1521-30 = 100) OF (——) THE EQUIVALENT OF THE WAGE RATE OF A BUILDING CRAFTSMAN EXPRESSED IN A COMPOSITE PHYSICAL UNIT OF CONSUMABLES; (----) THE QUANTITY OF FOODSTUFFS OBTAINABLE IN EXCHANGE FOR A UNIT QUANTITY OF INDUSTRIAL PRODUCTS.

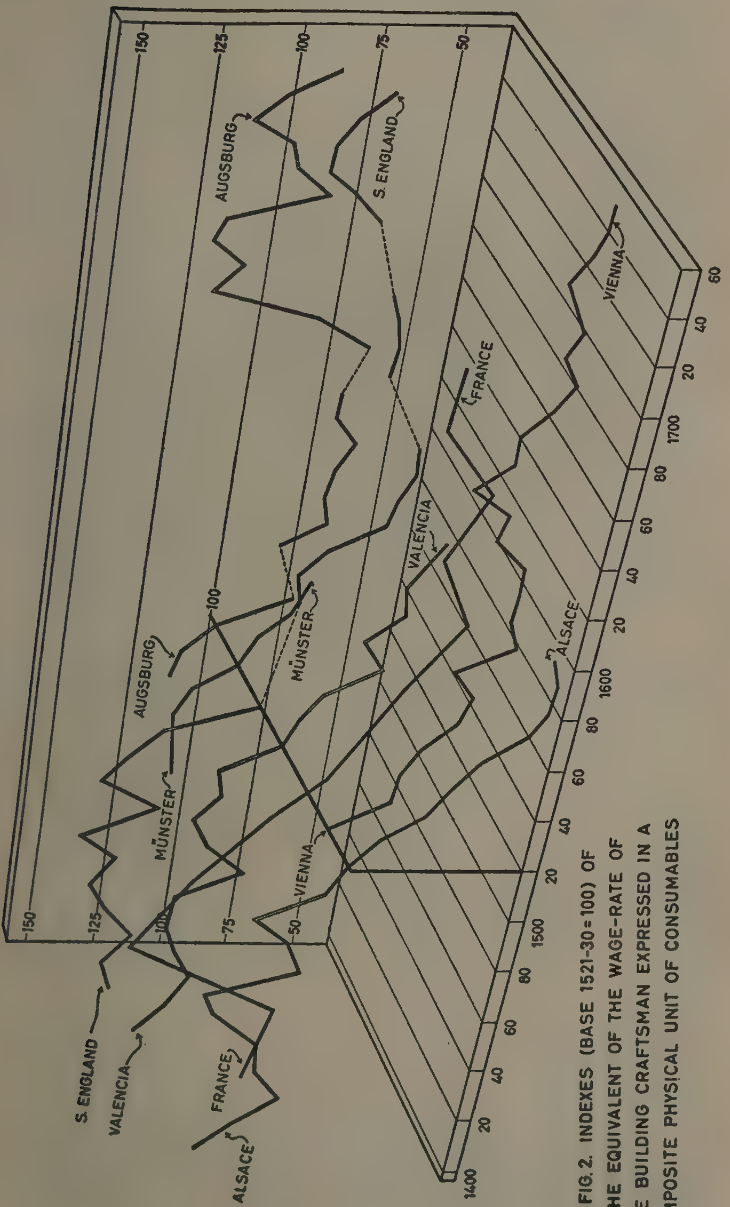

FIG. 2. INDEXES (BASE 1521–30 = 100) OF THE EQUIVALENT OF THE WAGE-RATE OF THE BUILDING CRAFTSMAN EXPRESSED IN A COMPOSITE PHYSICAL UNIT OF CONSUMABLES

percentage of professional labour included."[1] Some points in his statistical methods are not clear to us.[2] From his estimate of a threefold rise we can only come back to the Valencian builders' rates in his Appendix VII—the journeyman carpenter and journeyman mason, both 48 diners a day in 1502–05, 72 in 1599–1600; the master mason rising by absolutely as much but proportionately less, from 60 diners to 84; the mason's helper rising more, from 30 diners to 60, but still only doubling. The rise of a half which we take as representative is borne out by the agreement which appears here as elsewhere between the implied fall in the commodity equivalent of the wage, and the movement of the terms of trade between workshop and farm.

This movement of the terms of trade, which Fig. 1 illustrates for all four places, is the second salient feature of our findings. Again it agrees with what we found before in Southern England, France and Alsace. We can add now that it seems to have taken place in Sweden too. From the price indexes compiled by Dr. Ingrid Hammarström[3] for Stockholm and district we have made combined indexes of barley and butter on the one hand, and iron, bricks, lime, and cloth on the other. The price of barley, Dr. Hammarström says, was representative of that of rye, the most important bread grain, and we have given it twice the weight of butter; but our index for foodstuffs rests on rye and butter alone. When we compare it with that for the industrial products, we get the index of the terms of trade in the third column of Table III.

TABLE III

STOCKHOLM AND DISTRICT, 1460–1559: INDEXES OF (1) PRICES OF BARLEY (WEIGHT 2) AND BUTTER (WEIGHT 1); (2) PRICES OF OSMUND IRON, BRICKS, LIME AND NAARDEN CLOTH (AT EQUAL WEIGHTS); (3) QUANTITY OF BARLEY AND BUTTER OBTAINABLE IN EXCHANGE FOR A UNIT OF INDUSTRIAL PRODUCT, VIZ. (2) ÷ (1)
FROM DR. INGRID HAMMARSTRÖM'S MATERIALS
Base 1460–69 = 100

	(1)	(2)	(3)
1460–69	100	100	100
1470–79	99	107	108
1480–89	106	113	107
1490–99	128	98	77
1500–09	118	100	85
1510–19	164	112	68
1520–29	*372	*176	*47
1530–39	308	195	63
1540–49	500	231	46
1550–59	781	306	39

* 1525–29 only.

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[1] *Op. cit.*, p. 270.

[2] Viz. the adjustment, if any, for divergences between the purchasing powers of the units of account in different provinces; how changes in the array of series available from year to year were dealt with; the extrapolation for Andalusia in 1501–50, described in *op. cit.*, p. 272, n. 1; how indexes computed for periods without overlap were subsequently spliced.

[3] "The 'Price Revolution' of the Sixteenth Century: Some Swedish Evidence." *Scandinavian Economic History Review*, V. 2, 1957.

Evidently we are confronted with a movement of wide extent, and we have to look for an equally general cause. Our own suggestion has been that this lay in a growth of population. We can now add two pieces of evidence which tend to bear this out.

The first is the relationship between the population of certain German cities and food prices there, to which Elsas has drawn attention.[1] Admittedly the cities could have waxed and waned independently of population in the aggregate, and in the Thirty Years' War no doubt some did; but we know that the sixteenth century saw the growth of European cities generally,[2] and this may well have been the outlet that the growth of population found—when the death rate was so high in the cities, they could grow only through migration from the countryside. Elsas may therefore have been putting his finger on a factor of some generality when in his commentary on Frankfurt he said: " The lag of wages behind the rise of prices in the sixteenth century may be occasioned by the great accession to the labour force which the cities drew in from the countryside ".[3]

The second piece of evidence comes from France. Among the innumerable prices d'Avenel collected[4]—the last of all his tables contains six quotations for wooden legs—are the prices of land.[5] Much more goes to settling the price per hectare that any one parcel of land will fetch than the scarcity of land as a whole at the time relative to other factors of production; but when we aggregate many prices, as d'Avenel did, we can expect particularities to cancel one another out and let the prevailing influence of scarcity appear. This expectation gains support by the agreement between the movements of the averages d'Avenel presents separately for arable, pasture, vineyards and woodland: the price of woodland does not show so great a fall as the other prices between 1200 and 1475, and rises less afterwards; in both fall and rise pasture moves less than vineyards and arable; but with those exceptions the trends and the movements from one quarter-century to the next mostly agree. We have therefore made bold to form a combined index, with weights arable 6, pasture 2, vineyards 1, woodland 1; and compare it with our existing index for the building craftsman's wage-rate,[6] so as to see how the relative scarcities of land and at least one kind of industrial labour may have changed from time to time. The results are set out in Table IV and illustrated in Fig. 3, where the contemporary changes in the equivalent of the wage-rate in units of basic consumables[1] are also shown. It will be seen that from 1276 to 1700 the craftsman's wage generally rose relatively to the price of land when the basketful of consumables it would buy was also rising, and conversely; the most marked exception is in the quarter-century after the Black Death, and arises from a fall in the price of land then. The outcome is not unexpected, for in effect all we find now is that the price of land moved in sympathy with that of its produce; but it is something to have this direct indication of the changing scarcity of land writ large. It is generally consistent with the assumption that the growth of population in France was slower (it may even have been negative at times) between 1300 and 1475, more rapid thereafter until 1600, and then checked again.

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[1] M. J. Elsas, *Umriss einer Geschichte der Preise und Löhne in Deutschland*, vol. I (1936), pp. 77–82.

[2] Roger Mols, S. J., *Introduction à la démographie historique des villes de l'Europe du XIV^e au XVIII^e siècle* (Louvain, 3 vols. 1954–6), vol. II, Aperçu d'Ensemble; review by K. F. Helleiner, *Journal of Economic History*, March, 1958.

[3] *Op. cit.*, vol. II.B, p. 68.

[4] Vicomte G. d'Avenel: *Histoire économique de la propriété, des salaires, des denrées, et de tous les prix en général, depuis l'an 1200 jusqu'en 1800.* Vols. I, II, 1894; III, IV, 1898; V, 1909; VI, 1912; VI(b), ?; VII, 1926.

[5] *Op. cit.*, vol. II, p. 884.

[6] *Economica*, November, 1957, p. 305, Table 3.

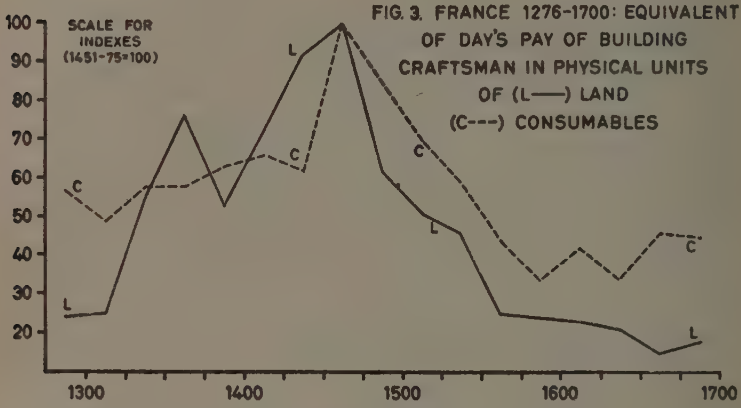

FIG. 3. FRANCE 1276-1700: EQUIVALENT OF DAY'S PAY OF BUILDING CRAFTSMAN IN PHYSICAL UNITS OF (L——) LAND (C---) CONSUMABLES

TABLE IV

FRANCE, 1276–1700: INDEXES OF (1) PRICES PER HECTARE (IN LIVRES TOURNOIS) OF FOUR KINDS OF LAND; (2) WAGE-RATE (IN LIVRES TOURNOIS) OF BUILDING CRAFTSMAN; (3) QUANTITY OF LAND OBTAINABLE IN EXCHANGE FOR A UNIT OF THE BUILDING CRAFTSMAN'S WORK, VIZ. (2)÷(1)
Base 1451–75=100

	(1)	(2)	(3)		(1)	(2)	(3)
1276–1300	131	31	24	1501–25 ..	218	112	51
1301–25 ..	166	41	25	1526–50 ..	338	156	46
1326–50 ..	90	50	55	1551–75 ..	728	179	25
1351–75 ..	106	81	76	1576–1600	1062	258	24
1376–1400	134	71	53	1601–25 ..	1188	268	23
1401–25 ..	124	89	72	1626–50 ..	1537	326	21
1426–50 ..	100	92	92	1651–75 ..	2635	405	15
1451–75 ..	100	100	100	1676–1700	2418	428	18
1476–1500	175	109	62				

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[1] Also from Table 3, *loc. cit.*

One other feature of the comparisons in Fig. 2 remains for notice: the three places for which we have estimates through the seventeenth and eighteenth centuries behave very differently then. In Vienna the craftsman's basketful increases at first after 1600, from less than half its bulk of the 1520's to nearly two-thirds about 1650, but then it slips back, and from the 1690's to 1760 it lies around the size it had been brought down to at its most meagre around 1610. In Southern England it not only recovers much as in Vienna down to 1650, but goes on increasing gently until the 1740's, when it is getting on for double what it had been at its worst, and is not much smaller than in the 1520's; but from 1750 it begins to shrink again. In Augsburg it behaves quite otherwise: with the end of the Thirty Years' War it doubles in a matter of only twenty years, and though it fluctuates in size thereafter, and its tendency is to shrink, it remains for most of a hundred years not far from its bulk of the 1520's. The contrast between Vienna and Augsburg seems the more striking at first because Augsburg had been stricken in the war as Vienna had not: but what mattered afterwards was not the rubble in the city itself but the terms on which it could now draw in supplies from the countryside around. In all three places the terms of this trade moved in close agreement with the changes in the size of the basketful, for which they provide a first explanation. They would themselves be consistent with our main conjecture, if in fact the population of southern Germany had been reduced through the Thirty Years War to little more than its figure of a hundred years before; whereas that of Lower Austria had kept up, and grew again somewhat after 1650, and that of Southern England grew more slowly if at all after 1600, until in the 1750's it began to rise rapidly again. According to Roger Mols,[1] the population of Augsburg doubled in a hundred years to reach 40–50,000 about 1600, but in 1645 it was only 21,000, and as late as 1790 it still did not exceed 50,000; whereas Vienna, in 1600 about the same size as Augsburg, had more than 100,000 inhabitants by 1700.

II

In Southern England we were also able to trace the labourer's rate over much of five centuries, and found that in settled periods from first to last down to 1914 it was commonly two-thirds of the craftsman's. This persistence of a rule of thumb, as we must think it, was remarkable. Does it appear elsewhere? In Vienna, yes: in the 1520's the tiler's and mason's labourers got six kronen a day against the nine kronen of the journeyman carpenter and mason; after 1750 the tiler's labourer got 18 to the journeyman's 27; and the ratio of two to three held pretty well throughout except that from 1670 onwards the mason's labourer lay somewhat below it. But in two other of our cities it seems to have been mostly otherwise: the differential was wider to start with, but then when rates generally rose it narrowed. In Valencia the prevailing rates down to 1550 seem to have been 48 for the journeyman against not 32 but 30 for the labourer; but then the labourer rose more, not relatively only but absolutely, to attain first three-quarters, and then by 1589 five-sixths, of the journeyman's rate. In Augsburg it is the mortar-mixer who is best recorded. His rate seems to have been higher by as much as a quarter than the mere labourer's, but even so was only four-sevenths of the journeyman mason's early in the sixteenth century; then his more rapid rise lifted the ratio by the 1570's to three-quarters or over. There it seems to have stayed until 1713, when it was shifted back to the two-thirds so familiar elsewhere. So in the eighteenth century the differential between the skilled and the unskilled was the same (at least for the mortar-mixer) in Augsburg as it was generally in Vienna and Southern England.

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[1] *Op. cit.*, vol. II, pp. 510–12.

III

Our present sources provide household budgets in Germany and Spain for comparison with the weights of the composite commodity that we adopted in the light of English evidence alone, but later used elsewhere with little change. The comparison is shown in Table V.

TABLE V

DIVISION OF OUTLAY DERIVED FROM GERMAN, SPANISH AND ENGLISH HOUSEHOLD BUDGETS

	Original		Common items only			
	Germany, (Palatinate) 1500–1700	Spain, 16th century	Germany, (Palatinate) 1500–1700	Spain, 16th century	S. England Davies & Eden, 1790's	Adopted by present authors
	(1) %	(2) %	(3) %	(4) %	(5) %	(6) %
1. Farinaceous	36	25	46	36	53	20
2. Meat, fish ..	11	23½	14	33½	12	25
3. Butter, cheese	9	2	12	3	7	12½
4. Drink ..	6	10	8	14	9	22½
Subtotal, Food ..	62	60½	80	86½	81	80
5. Fuel and light	8	5	10	7	7½	7½
6. Textiles ..	8	4½	10	6½	11½	12½
TOTAL, (1)–(6) ..	78	70	100	100	100	100
7. Other ..	22*	30†				
TOTAL, (1)–(7) ..	100	100				

* Other foodstuffs 6, shoes 4, rent 8, various 4.

† Miscellaneous, but not including rent.

Sources: Col. (1), G. Wiebe, *Zur Geschichte der Preisrevolution des XVI. und XVII. Jahrhunderts* (Leipzig, 1895), pp. 176–7. Col. (2), Earl J. Hamilton, *American Treasure and the Price Revolution in Spain, 1501–1650* (Harvard, 1934), pp. 275–6. Cols. (3) and (4): calculated by raising rows (1)–(6) in cols. (1) and (2) in equal proportions so as to make their totals 100. Cols. (5) and (6): *Economica*, November, 1956, p. 297.

It lends support to the view that we gave too little weight to breadstuffs. Our chosen proportions, it is true, are closely akin to those (not shown in the Table) of the British urban wage-earners' budgets of 1904, and (taking the two kinds of animal produce together) William Savernak's household in Dorset in the 1450's, but in the setting of seven centuries these were both times of exceptional prosperity. So we have recalculated our index of prices for Southern England with new weights for food more like cols. (3)–(5) of Table V:

	Original index %	Present re-calculation %
1. Farinaceous	20	50
2. Meat, fish	25	12½
3. Butter, cheese ..	12½	7½
4. Drink	22½	10
Subtotal, Food ..	80	80

But until 1855 the outcome proves to be so little changed that there is no need to set the new series out. It usually rises higher in a year of dearth, and drops lower in one of plenty, because grain fluctuated more widely than meat; but there is no divergence of trend, nor sustained difference of level, except that through most of 1659–85 the new index runs four or five per cent. above the old, and through 1818–46 often as much as ten per cent. below it. In 1855, however, a very different story begins, for the relative price of grain now falls steeply, and by 1895 the new index lies for the moment below 750 while the old is over 950. But this is a period in which the weights of the budgets of 1904 become increasingly appropriate.

There is a noteworthy implication of this fact that in the last hundred years meat has been so much dearer relatively to grain than in the six hundred years before: we must beware of supposing that wage-earners in those earlier years would have bought as little meat as the poor have in more recent times. " That island of England breeds very valiant creatures . . . give them great meals of beef, and iron and steel, they will eat like wolves, and fight like devils." It was possible to write that when the Tudor decline in the purchasing power of wages had already run most of its course.

The London School of Economics.

APPENDIX ON SOURCES AND METHODS

MÜNSTER

Source: G. Wiebe, *Zur Geschichte der Preisrevolution des XVI. und XVII. Jahrhunderts* (Leipzig, 1895).

Unit of Money: Some of the original quotations were in the golden gulden, some in silver marks, shillings, pence, and (later) thalers. Wiebe reduced them all to their contemporary equivalents in grams of gold, and our indexes follow his tables. Until 1539 Münster was effectively on a bimetallic standard, but as the weight of silver exchangeable for a unit weight of gold changed little, our indexes effectively show the movements of quotations in the unit of account. From 1539 onwards the silver thaler became the unit of account, and we are told that in 1545–60 silver depreciated relatively to gold by 2·7 to 6·7 per cent., so quotations in the unit of account would have risen to that extent more than our indexes.

Wage-rates: pp. 335–7, quotations mostly for wages when the employer provided food. After comparison mainly of series for carpenters, masons and tilers, the helpers of each, also sawyers and day labourers, we took out a representative series, whose movements differed little from those of the carpenter's wage, when food was provided, for the summer day.

Prices: (1) *Composite unit of consumables :* pp. 325–334. We followed the make-up of the English unit (*Economica*, November, 1956, p. 297, Table I). 1. *Farinaceous*. Rye, wheat and barley at equal weights, oats and peas at half weights. 2. *Meat and fish*. Fat oxen, calves, pigs, and fat sheep at equal weights, total subweight 21; herrings and stock-fish at equal weights, total subweight 4. 3. *Butter and Cheese* at equal weights. 4. *Drink*. Wine, malt and beer (average of five sorts) at equal weights. 5. *Fuel and light*. Tallow, tallow candles, and wax, sub-weights 1 each; charcoal, subweight 4½. 6. *Textiles*. Linen (average of four sorts) and cloth (average of five sorts) at equal weights.

(2) *Indexes of prices of foodstuffs and of industrial products*. The index for foodstuffs is provided by components 1–4 of the composite unit above, combined as there weighted. We made that for industrial products by combining at equal weights the series for linen and cloth in (6) above with others for salt, bricks, tiles, nails, paper, and men's shoes, pp. 330–4.

AUGSBURG

Source: M. J. Elsas, *Umriss einer Geschichte der Preise und Löhne in Deutschland*, vol. I (Leiden, 1936). All the extracts we used are from the accounts of the Augsburg hospital.

Unit of money: the Augsburg pfennig as unit of account throughout.

Wage-rates: We graphed the rates given at pp. 726–36 for mortar-mixers, unskilled building labourers, journeyman masons, and journeyman carpenters, and then took out by inspection a series representative of the craftsmen's rate.

Prices: (1) *Composite unit of consumables*. We followed the make-up of the English unit (*Economica*, November, 1956, p. 297, Table I) as far as possible, but until 1641 we had no entries for Drink, and transferred its weight to Farinaceous. We estimated the components as follows; the brackets after each commodity contain the weight we gave it in calculating the index for the whole component : 1. *Farinaceous*. Pp. 360–72, 383–5, 593–8, rye (8), barley (3), oats (3), peas (2), spelt (4 through 1673, 2 from 1674), and, from

1674 only, wheat (2). We spliced wheat to the mean of the others over 1674–85. 2. *Meat and fish.* Pp. 386–8, 392–6, 601–5, 607–14, beef (10), veal (5, transferred to beef in absence of veal 1499–1511, 1542–89), suet (3), dripping (3), carp (4, transferred *pro rata* to the other four in absence of carp before 1551). The entries for veal and suet were mostly of sales, those for the other three mostly of purchases. We spliced carp to the mean of the other four over 1551–64. 3. *Butter and cheese.* Pp. 390–1, 600–4, milk (5, transferred to cheese 1647–56, 1670–1711), and, from 1580 only, Dutch cheese (7½, transferred to milk until 1580). We spliced cheese to milk over 1580–95. 4. *Drink.* Pp. 405–6, 616, 618–20, brown beer. There are early entries ending with 1536, but we used the series only from its resumption in 1641, and until then transferred the weight of Drink to Farinaceous. We spliced brown beer to the mean of the series in Farinaceous over 1652–89. 5. *Fuel and light.* Pp. 408–10, 615–21, tallow (5), linseed oil (2½). 6. *Textiles.* Pp. 413–4, 615–20, twill: the quotations comprise different qualities.

(2) *Indexes of prices of foodstuffs and of industrial products.* The index for foodstuffs is given by components 1–4 of the composite unit of consumables, combined as weighted there. That for industrial products we made by combining at equal weights the series for textiles in component 6 above and the following: pp. 401–3, 418–25, 607–13, 615–20, 622–4, salt, Bavarian, in blocks; lime, by the barrel through 1573, by the peck from 1574—we spliced the second series to the first over 1557–74; bricks; writing paper.

VIENNA

Source: A. F. Pribram, *Materialien zur Geschichte der Preise und Löhne in Österreich*, vol. I (Vienna, 1938). Data here all from Vienna city hospital and the Klosterneuburg monastery, referred to here as WB and SK respectively.

Unit of money: the kreuzer as unit of account throughout.

Wage-rates: pp. 177–83, 344–50, 235–7, 515–20. Journeymen carpenters, masons and tilers in WB and SK; labourers of the last two in WB, and we are told that the SK rates for the mason's labourer were the same as the WB; tiler's labourer from 1636 at SK (p. 236, note H). Wage without board, for the summer day throughout, except that for the tilers there was no seasonal difference. By graphical inspection we took out a single rate as representative of the journeymen, mainly the carpenters and masons.

Prices: (1) *Composite unit of consumables.* We followed the make-up of the English unit (*Economica*, November, 1956, p. 297, Table 1), except that here we had no series for butter and cheese, whose weight we transferred to meat, and that we have had to compute the index before 1486 without any farinaceous component. There is no fish through 1607–91. We estimated the components as follows: 1. *Farinaceous.* SK 1486–1528, pp. 204–5, 447–9; WB 1527–1779, pp. 133–4, 269–74. Barley, " corn " (specified for SK as ' Traid, Korn, selten Roggen '), and wheat at equal weights throughout. We adjusted the quotations from harvest to calendar years by taking the price for the calendar year 1486 to be made up of half the prices for the harvest years 1485, 1486, and so on. We reduced the SK series of 1485–1528 by one-sixteenth to offset the larger SK bushel (p. 104), and took the adjusted series as continuous with the WB series which were all we used after 1528. Through 1690–1751 and again from 1752 onwards we adjusted prices to offset changes in the size of the bushel (pp. 101–2). 2. *Meat and fish.* Meat: pp. 215–6 and 465–72, 1445–1535, SK oxen (Hungarian), calves, whole sheep, half or quarter sheep; pp. 151–4 and 287–92, 1533–1735, WB oxen (Hungarian), sheep, with calves beginning only in 1565 and spliced to oxen over 1565–78. We joined the SK to the WB series as follows: we spliced SK oxen to WB at 1533-35, SK calves to SK oxen over 1516–33, and SK whole sheep to WB sheep at 1533–35, and we applied the factor calculated for this last splice to SK half or quarter sheep. We gave equal weights to the unweighted means of the series for cattle and sheep. Fish: pp. 220–1 and 471–6, SK herrings 1445–1533 and 1735–65, sturgeon 1445–1583 and 1694–1769; pp. 158 and 290–2, WB herrings 1528–1606 and 1692–1734. We spliced SK to WB herrings over 1528, 1531, 1533, and again over 1692–1729. We gave herrings and sturgeon equal weights throughout. We combined meat and fish with weights 33½ (including the 12½ transferred from butter and cheese) and 4. 4. *Drink*. Beer all WB, pp. 146–9 and 300–02, 307–8: 1527–1653 Iglauer, 1528–78 Freistädter, 1535–1616 Schweinitzer, 1583–1616 Regensberger, all prices of purchases; 1565–1653, Spital, ordinary, price of sales. We spliced the Regensberger to the Iglauer over 1595–1603. The Spital stands on its own base in 1535–44, though we have not included its entries at that time in the index. Wine, SK, pp. 213–4 and 462–4, 1450–1750 sales, 1483–1748 retail; WB, pp. 143–5 and 308–11, 1526–1756 sales, 1526–1770 retail, 1592–1770 purchases. The WB wines are all heuriger (first-year) and the prices are for the harvest year: we entered the price for harvest year 1525 against calendar year 1526, and so on. We spliced SK to WB, retail over 1526–33, sales over 1553–87; and WB purchases to retail over 1630–62. We combined the unweighted means of beer, wine with weights 3 and 1. 5. *Fuel and light*. Fuel: pp. 163–4, 295–7, 321–6, WB, 1527–1603 and 1634–1779, soft firewood, with fir (p. 332) used for interpolation 1746–62; 1551–1665, charcoal. We spliced charcoal to firewood over 1582–97. We combined the two series with equal weights to give an index for fuel. Light: pp. 171–3, 311–4, 316, 327, 335–8, WB, tallow candles, (i) with black wick, 1439–1512, 1553–84, 1630–1781, (ii) with cotton wick 1630–1781, raw tallow 1527–58; pp. 233, 508–11, SK tallow candles, 1448–1582. We spliced candles (i) to raw tallow over 1555–8, and (ii) to (i) over 1680–1702. We combined these series at equal weights to get an index for candles. Pp. 232, 508–14, SK olive oil, 1442–1532, 1646–1771; pp. 170–1, 316, WB olive (lighting) oil, 1722–79. We spliced SK oil to SK candles over 1520–9, and where there were two series for oil combined them at equal weights so as to have a single index for oil throughout. We combined the three indexes with weights fuel 5, candles 4, oil 1. 6. *Textiles*. Pp. 222–3, 499–501, SK hemp, 1443–1532, adjusted from harvest to calendar years; pp. 168–9, 321–30, WB thread (cheap sort) 1527–1703, coarse woollen cloth 1527–1778. We spliced hemp to thread at 1527, 1531, and formed an index with weights hemp and thread combined 1, cloth 3.

(2) *Indexes of prices of foodstuffs and of industrial products*. The index for foodstuffs is provided by components 1–4 of the composite unit above, combined as there weighted. That for industrial products we made by combining with equal weights the indexes for components 5 and 6 above with indexes for building materials and ironwork which we formed as follows. *Building materials:* the following six indexes combined at equal weights—*bricks*, mean of WB (without delivery charge) pp. 162, 586, and SK pp. 224, 630; *tiles* (without delivery charge), WB, pp. 162, 586; *shingles*, mean of WB, pp. 165, 586, 8, and SK, small, pp. 225, 630; *lime*, WB, pp. 161, 586, combined at equal weights with mean of SK series 1, 2, 4, pp. 223–4, 628;

pitch, WB, pp. 174, 592, 4; *timber*, unweighted mean of WB ordinary planks, carpenter's planks, bench planks, and laths, pp. 165, 586, 588. *Ironwork:* unweighted mean of three SK series for *nails*—large shingle, partition, large lath, pp. 228–9, 634.

VALENCIA

Sources: Earl J. Hamilton, (M) *Money, Prices and Wages in Valencia, Aragon, and Navarre, 1351–1500* (Harvard, 1936); (A) *American Treasure and the Price Revolution in Spain, 1501–1650* (Harvard, 1934).

Unit of money: the diner as unit of account throughout.

Wage-rates: We graphed the quotations for the following: 1392–1500, carpenters, masons and stonecutters, masters and helpers, with journeyman masons, and unskilled labourers; 1501–1600, carpenters, masons and stonecutters, masters and journeymen, with mason's helper to 1650. By inspection we took out a rate representative of the movements of the craftsmen's rates generally, and at the level of the master carpenter's rate down to 1500, the journeyman carpenter's thereafter; we joined the two by assigning to the first entry of the second the same index-number value as appeared for the last entries of the first. We also took out a series representative of the carpenters' and masons' helpers with the unskilled labourers in 1392–1500, and continued this without any change of rate at 1500, but now on the showing of the mason's helper alone, down to 1638.

Prices: (1) *Composite unit of consumables.* We followed the make-up of the English unit (*Economica*, November, 1956, p. 297, Table 1) but because Drink meant wine not beer gave it a weight of only 12½ and transferred the rest of its English weight to Farinaceous, whose weight becomes 30. We estimated the components as follows. Prices are from M, Appendix III, and A, Appendix III.D, IV.D, V.D. 1. *Farinaceous.* We spliced carob beans and rice to wheat over 1520–60. 2. *Meat and fish.* We took the predominant values of beef, mutton and hens in the 1440's as an approximation to the required base in 1451–75, when wheat prices lay about the same level as in the 1440's. We spliced lard to mutton over 1505–30, and cod to tuna over 1558–68. Sub-weights: before 1451, beef 8, mutton 8, hens 5; before 1501, sardines 1, tuna 1½, hake 1½; from 1501, mutton 13, hens 4, lard 4, tuna 2, dried cod 2. 3. *Butter and cheese.* Cheese only. In the absence of any series even for cheese through 1551–1601, we extrapolated the index for all foodstuffs in 1550 in proportion to the combined movement of the remaining foodstuffs. 4. *Drink.* Wine only. 5. *Fuel and light.* We spliced charcoal to firewood over 1520–40. Sub-weights, before 1501, firewood 5, olive oil 2½; from 1501, firewood 4, charcoal 3½. 6. *Textiles.* We spliced linen and bedticks to flax over 1552–66. Sub-weights from 1552, flax 3, linen 3, bedticks 6½.

(2) *Indexes of prices of foodstuffs and of industrial products.* The index for foodstuffs is provided by components 1–4 of the composite unit above, combined as there weighted. We made that for industrial products by combining at equal weights firewood, plaster of Paris, lime, bricks, and (not after 1500) nails, large.

TABLE VI

BUILDERS' WAGE-RATES IN MÜNSTER, AUGSBURG, VIENNA AND VALENCIA, FOR VARIOUS PERIODS WITHIN 1413–1796

1. MÜNSTER. Index of craftsman's rate for summer day, in units of gold; base 1467–1500, when the rate for the carpenter, summer day, employer providing food, was about 0·192 grams gold, that is, about 0·072 gulden, or 1·4 schilling.

1467–1500	100	1531–40	84
1501–10	80	1541–50	79
1511–20	83	1551–60	74
1521–30	77		

2. AUGSBURG. (1) Craftsman's rate as represented mainly by the mason, in pfennig for summer day. (2) Unskilled rate as represented mainly by the mortar-mixer, in pfennig per day, summer and winter not distinguished. Both apparently without board.

(1)		(2)		(1)		(2)	
	d.		*d.*		*d.*		*d.*
1499–1538	28	1502–19	16	1604–21	63	1606–24	n.a.
1539–52	n.a.	1520–37	n.a.	1622–29	n.a.	1625–30	52·5
1553–68	35	1538	19	1630–44	77	1631–52	n.a.
1569–1603	45·5	1539–52	n.a.	1645–65	84	1653–66	63
		1553–65	24·5	1666–67	n.a.	1667–74	n.a.
		1566–7	n.a.	1668–1712	91	1675–1763	70
		1568	30·5	1713–96	105		
		1569–72	31·5				
		1573	34·5				
		1574–1605	35				

3. VIENNA. Craftsman's rate, represented mainly by masons and carpenters, in kronen for a summer day, without board.

	Kr.		*Kr.*
1520–43	9	1641–69	22
1544–74	10	1670–82	19·5
1574–79	Rising to 12	1682–90	Rising to 24
1579–1600	12	1690–1711	24
1600–02	Rising to 15	1712–20	24·5
1602–18	15	1721–79	25·5
1618–41	Rising to 22		

4. VALENCIA. Index of craftsman's rate, represented mainly by the master and journeyman carpenters, in diners per day (the rate was the same throughout the year) without board for the most part. Base 1451–75 = 100, when the master carpenter's rate was 57d.

1392–1463	105	1561–78	132
1463–73	Falling to 89	1579–85	145
1473–87	89	1586–1607	158
1488–1560	105		

TABLE VII

INDEXES (1521–30=100) OF (1) PRICE OF COMPOSITE UNIT OF CONSUMABLES, (2) EQUIVALENT OF WAGE-RATE OF BUILDING CRAFTSMAN, EXPRESSED IN THE ABOVE PHYSICAL UNIT, IN MÜNSTER (M), AUGSBURG (A), VIENNA (W) AND VALENCIA (V), FOR PERIODS WITHIN 1413–1770

(*a*) MÜNSTER 1467–1560

	M.1	*M.2*		*M.1*	*M.2*		*M.1*	*M.2*		*M.1*	*M.2*
1467–1500	90	143	1511–20	90	121	1531–40	113	97	1551–60	137	70
1501–10	81	130	1521–30	100	100	1541–50	123	84			

(*b*) VALENCIA 1413-98 (*contd. in* (*c*))

	V.1	*V.2*		*V.1*	*V.2*		*V.1*	*V.2*		*V.1*	*V.2*
1413	111	90	1435	91	110	1457	81	122	1479	87	97
1414	78	128	1436	88	113	1458	71	141	1480	84	101
1415	77	130	1437	90	111	1459	76	131	1481	67	126
1416	64	156	1438	91	109	1460	75	132	1482	74	114
1417	67	149	1439	87	115	1461	69	144	1483	69	122
1418	69	144	1440	85	117	1462	77	130	1484	76	111
1419	71	141	1441	76	131	1463	76	131	1485	100	85
1420	72	138	1442	84	118	1464	77	128	1486	77	110
1421	80	125	1443	81	123	1465	70	138	1487	76	111
1422	78	127	1444	81	123	1466	68	139	1488	74	134
1423	74	134	1445	81	122	1467	73	129	1489	76	131
1424	75	132	1446	82	121	1468	73	127	1490	75	132
1425	79	126	1447	83	120	1469	75	121	1491	86	116
1426	84	119	1448	71	141	1470	83	108	1492	78	128
1427	93	107	1449	79	126	1471	79	111	1493	81	122
1428	82	121	1450	77	130	1472	74	116	1494	84	118
1429	71	139	1451	79	126	1473	83	102	1495	79	126
1430	83	120	1452	81	123	1474	101	84	1496	82	121
1431	78	128	1453	74	134	1475	84	101	1497	80	125
1432	81	122	1454	70	142	1476	68	123	1498	82	121
1433	79	126	1455	73	136	1477	69	122			
1434	75	132	1456	81	123	1478	82	103			

(*c*) AUGSBURG 1499–1753, VIENNA 1520–1770,
VALENCIA (*contd.*) 1499–1607

	A.1	*A.2*	*W.1*	*W.2*	*V.1*	*V.2*		*A.1*	*A.2*	*W.1*	*W.2*	*V.1*	*V.2*
1499	77	122	91		76	131	1514	77	124	91		79	126
1500	97	97	79		71	141	1515	88	108	92		81	123
1501	100	95	79		70	142	1516	102	93	86		91	109
1502	89	107	80		96	104	1517	100	95	86		87	114
1503	96	99	86		114	87	1518	81	117	86		84	119
1504	93	102	91		84	119	1519	89	107	89		85	117
1505	74	129	87		87	114	1520	85	111	87	112	79	126
1506	73	130	86		84	118	1521	80	119	86	114	111	89
1507	77	124	81		84	119	1522	83	115	84	118	115	86
1508	80	119	75		82	121	1523	81	117	86	114	91	110
1509	92	103	86		82	121	1524	86	110	91	108	92	108
1510	92	103	87		85	117	1525	77	124	104	95	98	101
1511	96	99	86		85	117	1526	104	91	109	90	94	105
1512	80	119	80		81	122	1527	98	96	99	99	101	99
1513	79	120	90		82	121	1528	94	100	100	98	99	100

	A.1	*A.2*	*W.1*	*W.2*	*V.1*	*V.2*
1529	150	63	102	95	97	102
1530	147	64	141	69	101	98
1531	144	66	124	79	104	95
1532	140	68	130	75	106	94
1533	160	59	141	69	101	99
1534	155	61	127	77	107	93
1535	120	79	127	77	103	97
1536	100	95	125	79	104	96
1537	121	79	125	79	98	101
1538	121	79	124	79	104	95
1539	121		115	85	108	92
1540	154		115	85	109	91
1541	124		127	77	107	93
1542	136		130	75	111	89
1543	151		122	80	102	97
1544	146		135	80	98	101
1545	138		156	70	111	89
1546	133		166	66	126	79
1547	131		147	75	106	94
1548	126		130	83	115	86
1549	149		122	89	126	79
1550	170		149	73	130	77
1551	185		156	70	130	76
1552	163		157	69	145	69
1553	156	76	155	71	157	63
1554	144	82	130	83	148	67
1555	143	83	151	72	146	68
1556	136	87	156	65	146	68
1557	144	83	158	69	175	57
1558	155	76	152	71	153	65
1559	185	64	165	67	151	66
1560	173	69	217	50	150	66
1561	168	71	185	59	150	84
1562	180	66	166	65	156	80
1563	171	70	172	64	163	77
1564	165	72	170	64	153	82
1565	176	68	172	64	170	73
1566	206	58	176	62	176	71
1567	209	57	183	60	169	74
1568	173	69	191	57	182	69
1569	232	67	222	49	180	70
1570	351	44	268	41	187	67
1571	303	51	277	40	196	64
1572	255	60	260	42	177	70
1573	272	57	220	50	203	62
1574	290	53	216	51	212	59
1575	197	79	221	52	210	60
1576	191	81	203	58	208	60
1577	192	80	177	69	190	66
1578	210	73	187	68	216	58
1579	235	66	182	71	211	65
1580	225	69	210	63	205	67
1581	202	76	203	64	210	65
1582	205	76	185	71	223	62
1583	187	82	200	65	240	57
1584	223	69	201	65	233	59
1585	245	62	208	63	219	63
1586	286	53	210	63	199	75
1587	265	58	207	64	217	69
1588	257	60	201	65	229	65
1589	310	49	208	63	244	61
1590	292	52	241	55	226	66
1591	253	61	247	53	239	62
1592	235	66	213	61	264	57
1593	255	60	232	56	263	57
1594	265	58	258	51	242	62
1595	238	64	286	46	236	63
1596	224	69	257	51	222	68
1597	258	59	266	49	241	62
1598	288	53	295	44	272	55
1599	262	59	336	39	295	51
1600	305	50	389	33	276	54
1601	295	52	354	41	285	52
1602	292	52	332	49	287	52
1603	272	57	347	47	315	48
1604	261	82	333	49	290	52
1605	272	79	290	56	277	54
1606	284	75	321	51	262	57
1607	288	74	356	46	249	60
1608	286	74	322	49	245	
1609	305	70	324	50	246	
1610	320	67	308	53	262	
1611	330	64	305	53	256	
1612	326	66	298	55	268	
1613	318	68	314	52	269	
1614	369	58	383	43	285	
1615	332	64	348	47	250	
1616	322	67	347	47	259	
1617	285	75	337	48	285	
1618	268	80	361	45	285	
1619	299	72	366	45	265	
1620	323	67	494		263	
1621	567	37	523		265	
1622	1364		955		264	
1623	574		1901		288	
1624	467		830		269	
1625	539		597		265	
1626	502		409	47	293	
1627	472		438	44	294	
1628	468		406	48	302	
1629	387		378	53	296	
1630	339	76	429	48	320	
1631	312	84	353	59	327	
1632	474	55	329	64	315	
1633	788		334	64	304	
1634	1072		343	64	309	
1635	830	32	368	60	306	
1636	583	45	429	52	289	
1637	483	55	429	53	356	
1638	399	66	482	48	328	
1639	354	74	441	53	286	
1640	376	70	387	62	278	
1641	398	66	388	62	288	
1642	356	73	411	59	292	
1643	327	80	424	56	308	
1644	301	86	448	53	385	
1645	296	96	492	48	318	
1646	352	81	438	55	313	
1647	355	80	454	53	351	
1648	425	67	427	56	297	
1649	484	59	416	58	342	
1650	337	84	414	58	328	

	A.1	*A.2*	*W.1*	*W.2*
1651	285	100	392	61
1652	291	98	372	64
1653	278	103	389	62
1654	226	126	378	64
1655	225	127	382	63
1656	238	119	312	77
1657	324	121	364	66
1658	242	118	317	75
1659	248	115	292	82
1660	275	104	333	72
1661	332	85	372	64
1662	317	90	396	60
1663	315	91	468	52
1664	312	92	515	47
1665	278	103	570	42
1666	267		432	56
1667	260		432	56
1668	255	121	367	65
1669	249	124	316	76
1670	233	132	349	61
1671	226	136	337	63
1672	227	135	327	65
1673	237	130	328	65
1674	266	116	331	64
1675	285	108	352	60
1676	282	109	341	63
1677	269	115	394	54
1678	285	108	368	58
1679	302	103	428	50
1680	314	98	353	60
1681	279	110	311	68
1682	260	119	329	64
1683	266	116	369	60
1684	283	109	533	42
1685	245	126	613	37
1686	247	124	572	41
1687	255	121	587	41
1688	291	106	499	50
1689	312	98	569	45
1690	306	100	515	51
1691	370	83	507	52
1692	413	75	497	52
1693	481	64	515	51
1694	397	78	574	45
1695	332	93	616	42
1696	302	103	603	44
1697	307	100	665	39
1698	372	83	656	40
1699	444	70	638	41
1700	379	81	755	35
1701	377	82	706	37
1702	346	90	636	41
1703	381	81	540	48
1704	386	80	469	56
1705	332	93	492	53
1706	300	103	497	52
1707	296	105	472	56
1708	321	96	493	53
1709	349	88	542	48
1710	339	91	567	46
1711	377	82	487	54
1712	433	71	560	48
1713	366	97	579	46
1714	362	98	615	44
1715	362	98	612	44
1716	387	92	603	44
1717	352	100	599	44
1718	339	105	668	40
1719	353	100	689	39
1720	354	100	621	43
1721	339	105	592	47
1722	326	109	509	55
1723	325	109	462	60
1724	344	104	437	64
1725	333	107	500	56
1726	352	102	532	52
1727	324	110	578	48
1728	335	106	534	52
1729	335	106	507	55
1730	324	110	480	58
1731	355	100	498	56
1732	323	110	507	55
1733	314	114	490	56
1734	317	112	487	57
1735	345	104	513	54
1736	362	98	483	57
1737	375	95	544	51
1738	393	91	553	50
1739	409	87	583	48
1740	495	72	633	44
1741	473	75	624	44
1742	469	76	593	47
1743	440	81	644	43
1744	393	91	582	48
1745	422	84	593	47
1746	436	82	633	44
1747	393	91	582	48
1748	383	93	553	50
1749	405	88	519	53
1750	552	64	470	59
1751	552	64	456	61
1752	525	68	562	49
1753	526	68	572	48
1754			592	47
1755			567	49
1756			621	44
1757			739	37
1758			847	33
1759			787	35
1760			615	45
1761			594	47
1762			614	45
1763			597	47
1764			609	46
1765			603	46
1766			656	42
1767			673	41
1768			680	41
1769			630	44
1770			670	41

TABLE VIII

INDEXES OF (1) PRICE OF A COMPOSITE UNIT OF FOODSTUFFS, (2) PRICE OF A SAMPLE OF INDUSTRIAL PRODUCTS, (3) QUANTITY OF FOODSTUFFS COMMANDED IN EXCHANGE BY A UNIT OF INDUSTRIAL PRODUCTS IN OUR SAMPLE, I.E. (2) ÷ (1), DECENNIAL AVERAGES

1521–30 = 100

	MÜNSTER			AUGSBURG			VIENNA			VALENCIA		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1411–20										76	126	166
1421–30										80	122	153
1431–40										86	118	136
1441–50										80	127	159
1451–60							69	79	115	77	114	148
1461–70							81	84	104	74	107	144
1471–80							79	88	111	83	108	130
1481–90	*87	*99	*114				86	101	117	78	93	119
1491–1500							89	98	110	83	89	108
1501–10	78	91	117	83	115	138	79	99	125	89	88	99
1511–20	89	101	113	84	101	121	86	91	106	82	95	116
1521–30	100	100	100	100	100	100	100	100	100	100	100	100
1531–40	114	106	93	134	130	97	128	108	84	107	102	96
1541–50	126	113	90	140	136	97	142	110	77	116	107	92
1551–60	148	115	78	157	140	89	168	114	68	158	134	84
1561–70				208	165	79	202	131	65	177	154	87
1571–80				243	180	74	232	134	58	218	159	73
1581–90				258	185	72	219	140	64	239	178	74
1591–1600				274	208	76	296	166	56	279	202	72
1601–10				292	240	82	349	205	59	296	236	80
1611–20				330	233	71	387	201	52	293	226	77
1621–30				—	—	—	—	—	—	311	234	75
1631–40				625	303	48	427	248	58	337	254	75
1641–50				380	317	83	477	253	53	355	270	76
1651–60				260	318	122	382	248	65			
1661–70				289	317	110	466	244	52			
1671–80				287	290	101	392	221	56			
1681–90				289	311	108	549	294	54			
1691–1700				400	295	74	684	306	45			
1701–10				359	344	96	598	304	51			
1711–20				394	305	77	675	309	46			
1721–30				361	320	89	569	274	48			
1731–40				398	—	—	613	270	44			
1741–50				464	392	84	674	310	46			
1751–60				587	—	—	753	300	40			
1761–70							755	329	44			
1771–80							730	342	47			

* average of 1467–1500

The Physiocratic Concept of Profit[1]

By Ronald L. Meek

I

The most significant theoretical advance which Adam Smith made over the work of his predecessors was undoubtedly his inclusion of profit on capital as a constituent element in the supply price of commodities. Before Smith, speaking very broadly, " profit " had usually been regarded either as a mere synonym for " gain ", or as a sort of superior wage, or as a surplus over cost whose level varied (as Steuart put it) " according to circumstances ".[2] With one bold stroke Smith cut through the difficulties involved in these earlier approaches to the problem. He postulated profit as the income of the class of employers of labour—the third of " the three great, original and constituent orders of every civilized sociery ";[3] he argued that competition would tend to reduce this profit to " an ordinary or average rate " on the capital employed;[4] and he included profit at this " natural " rate along with wages and rent (at *their* " natural " rates) in the supply price of commodities.[5] What was more, he was careful to distinguish profit from wages of management,[6] and to emphasise that after sufficient had been set aside as a compensation for " the occasional losses to which every employment of stock is exposed " and for payment of interest to the lender (if any), there still normally remained a " neat or clear " surplus, out of which new capital could be accumulated.[7] Profit, in other words, was not only a constituent part of the supply price of commodities: it also constituted, on an aggregative view, an important part of the " net revenue " of society.

The present article discusses the contribution made by the French Physiocrats to the development of this Classical concept of profit. The main problem here revolves around an apparent contradiction in the work of the Physiocrats which has never been properly resolved and which can be stated as follows: The Physiocrats stoutly maintained not only the plausible thesis that land rent was an income in the nature of a surplus, but also the much less plausible thesis that land rent was the *only* income in the nature of a surplus, all other returns to agents of production being merely compensation for costs.[8] Yet at the same time their theoretical system can properly be said to have been a "capitalist" system,[1] in the sense that its whole *raison d'être* was the advocacy of a state of affairs in which economic activity, particularly in agriculture, would be conducted by wealthy entrepreneurs motivated by a desire for profit. References to the large surpluses over cost obtainable by entrepreneurs under certain circumstances abound in the Physiocratic literature, from the first days of the School to the last. How then, if at all, can these two aspects of the Physiocratic system be reconciled? In what follows it will not be denied that in a certain sense the contradiction was a real one. It will be suggested, however, that in the work of the founders of the School (Quesnay and Mirabeau) the contradiction remained latent owing to their adoption of certain assumptions concerning the nature and sources of profit; that the contradiction emerged to the surface only when economic developments rendered these assumptions implausible; and that the later history of Physiocratic theory consisted largely of attempts to solve the contradiction, in the course of which substantial advances towards the Classical concept of profit were made.

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[1] A translation in Polish of this article is appearing in the journal *Ekonomista*.

[2] Steuart, *Political Oeconomy* (1767), vol. I, p. 182.

[3] *Wealth of Nations* (Cannan edn.), vol. I, p. 248.

[4] *Ibid.*, p. 57.

[5] *Ibid.*, pp. 57–8.

[6] *Ibid.*, pp. 50–51.

[7] *Ibid.*, pp. 97–9.

[8] Cf. Schumpeter, *History of Economic Analysis*, p. 238.

II

Let us consider first the place which the "profit" of the agricultural entrepreneur occupied in Quesnay's theoretical system. The best starting-point here is the *Analyse du Tableau Économique*, which first appeared in the June, 1766, number of the *Journal de l'Agriculture, du Commerce et des Finances*, and which was reprinted two years later, in amended and augmented form, in Du Pont's *Physiocratie*.[2] In the *Analyse*, as usual in Physiocratic writing, the nation is divided into three socio-economic classes: the productive class (those engaged in agriculture, including both entrepreneurs and wage-earners); the class of proprietors (the landlords, sovereign and clergy, who receive the *produit net* in the form of rent, taxes and tithes respectively); and the sterile class (those engaged in non-agricultural employments, including both entrepreneurs and wage-earners).[3] The nation in question is assumed to be one in which the main reforms advocated by the Physiocrats have been adopted, namely, the universal application of large-scale capitalist agriculture;[4] the establishment of free trade in corn, which ensures a *bon prix* for agricultural produce;[5] the institution of the *impôt unique*, which frees the agricultural entrepreneur from tax burdens;[6] and the complete fulfilment by the class of proprietors of their duty to provide the *avances foncières* for the improvement of their land.[7] Under these conditions, it is assumed that the productive class, employing *avances primitives* of 10 and *avances annuelles* of 2, will produce a harvest of 5. This harvest will be sufficient, after setting aside 2 for the replacement of the *avances annuelles* and 1 for "*intérêt*" on the *avances primitives*, to yield a *produit net* or net revenue of 2 to the class of proprietors. On the basis of these assumptions, the *Tableau* shows how the flow of income between the three classes will result in the needs of each being satisfied. In the particular case analysed by Quesnay, agriculture, wealth and population remain in the same state without increase or decline (i.e., net investment is zero); but he hastens to add that a different set of data will produce a different result.[1]

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[1] Cf. Weulersse, *Le Mouvement Physiocratique*, vol. II, p. 684.

[2] Quotations in this article are from the latter version, as reprinted in Oncken, *Oeuvres de Quesnay*.

[3] Oncken, pp. 305–9.

[4] *Ibid.*, p. 309.

[5] *Ibid.*, pp. 309 and 311.

[6] *Ibid.*, p. 311.

[7] *Ibid.*, pp. 318–9.

In all this there is no specific mention of the "profit" of the agricultural entrepreneur. How then does this "profit" fit into Quesnay's theoretical scheme? It is possible, of course, that Quesnay simply forgot about the existence of this type of income when writing the *Analyse*, or that he regarded it as quantitatively insignificant; but in view of the great emphasis laid on agricultural profit in Quesnay's work taken as a whole (and particularly in his early *Encyclopædia* articles) this seems hardly likely. There are therefore four possibilities: (1) that the farmer's profit is included in the *produit net*; (2) that it is included in the "*intérêt*" received by the farmer on his *avances primitives*; (3) that under the conditions postulated in the *Analyse* no profit would in fact be earned by the farmers; and (4) that it is included in the *reprises annuelles* along with the ordinary costs of cultivation.

The first possibility might at first sight hardly seem worth-while exploring, in view of the Physiocrats' persistent and very positive identification of the *produit net* with the income of the class of proprietors. Yet at least twice in Quesnay's early work, and (perhaps) once in his later work, this identification breaks down, and the "profit" of the farmer is included, along with the rent of the landlord, in the *produit net*. The first occasion on which this occurs is in *Grains*, where Quesnay is discussing what he calls the "*état de la grande culture des grains*" which exists in certain northern provinces of France. The *produit net* per *arpent* of land cultivated in this manner is estimated by Quesnay at 17 liv. 8s. Out of this sum the farmer must pay his rent, and "*il doit aussi payer la taille sur cette même somme et y trouver un gain pour subsister*".[2] So the 17 liv. 8s. is divided up as follows:

For the proprietor	3/5, or	10	7	7	17 liv. 8s.
For the *taille* ..	1/5, or	3	9	6	
For the farmer ..	1/5, or	3	9	6	

Quesnay adds in a footnote that he is not basing his calculation on the actual level of the *taille*, but rather assuming an assessment which "*laisse quelque profit au fermier et un revenu au propriétaire*". In this context, then, the *produit net* is evidently being defined so as to include not only the landlord's rent and the *taille* but also a " *gain pour subsister* " or " *profit* " for the farmer.

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[1] *Ibid.*, p. 316.
[2] *Ibid.*, p. 198.

The second occasion is in the *Extrait des Oeconomies Royales de M. de Sully*, appended by Quesnay to an early edition of the *Tableau*.[1] The eighth maxim reads as follows: " 8° *Que les avances des fermiers soient suffisantes pour que les dépenses de la culture reproduisent au moins cent pour cent:*[2] *car si les avances ne sont pas suffisantes, les dépenses de la culture sont plus grandes à proportion, & donnent moins de produit net* ". To this there is appended the following interesting note:

> *Dans tel Royaume les avances ne produisoient du fort au foible, l'impôt à part, qu'environ 20 pour cent, qui se distribuoient à la dixme, au propriétaire, au fermier, pour son gain, les intérêts de ses avances, & ses risques: Ainsi deficit de trois quarts sur le produit net.*[3]

Here Quesnay is even more explicit than in the previous instance.

The third occasion, where the proper interpretation is rather more doubtful, is in the dialogue *Sur les Travaux des Artisans* (1766). There are two relevant passages. If there is a diminution in the wages cost of cultivation, says Quesnay in the first, then " *le profit de l'épargne des dépenses en frais serait consacré naturellement par les cultivateurs à l'accroissement de leurs travaux, qui augmenteraient les productions et le revenu* ".[4] In the second, which occurs a few pages later, Quesnay says:

> *Vous voyez donc qu'il faut distinguer en deux parties la reproduction que le cultivateur a fait naître; savoir, celle qui est pour sa propre subsistance, et celle qui excède cette même subsistance. D'où suit que, si l'on peut, sans préjudicier à la reproduction totale, restreindre la première portion, on accroît d'autant la seconde. Par exemple, supposé que la reproduction soit vingt, la dépense du cultivateur dix et l'excédent dix; si la dépense peut être restreinte à huit, l'excédent sera douze.*[5]

If one can legitimately identify the " *excédent* " in the latter passage with the *produit net*, then if the two passages quoted are read together it seems to follow that in Quesnay's view a reduction in the wages bill would allow the farmer to share in the *produit net*.

The three sets of remarks which I have quoted are interesting enough, but I think it would be wrong to put too much weight on them. For one thing, they are very isolated: I have not been able to find any others of similar import in Quesnay's work. Then again, the first is very early;[6] the second seems to occur in this particular version of the

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[1] I refer to the edition which was reprinted by the British Economic Association in 1894.

[2] The quantitative relationship referred to here is that between the *avances annuelles* and the *produit net*.

[3] B.E.A. reprint, p. 4.

[4] Oncken, p. 545.

[5] *Ibid.*, p. 551.

[6] *Grains* appeared in the *Encyclopædia* in 1757.

Maximes only;[1] and the interpretation of the third, as I have said, is rather doubtful. However, a point of some importance is suggested by the first and third of the quotations, if not by the second. The first, it may be noted, refers to a situation where profits are being earned by a relatively small group of agricultural entrepreneurs who have as it were stolen a march on their rivals in the introduction of technical innovations. The third refers to a situation where profits are being earned through economies in the wages bill. Now it would be perfectly reasonable to argue that profits of this character constituted a part of the net revenue of society at any given moment, while at the same time arguing that in the long run these profits must either disappear or crystallise out into rents for the proprietors. Thus it is not necessarily inconsistent to say in one place that profits of this type are included in the *produit net*, and in another place, where a more long-period view is being taken, to say that they are not so included. Further evidence will be offered later to suggest that Quesnay did in fact think of the problem of agricultural profit at least partly in such terms as these.

Consider now the second possibility mentioned above—that Quesnay conceived the " profit " of the agricultural entrepreneurs to be included in the " *intérêt* " they received on their *avances primitives*. This possibility must, I think, be definitely rejected. Quesnay was by no means the most consistent of writers, but the general impression one receives from his comments on the farmer's " *intérêt* " taken as a whole is that he regarded it basically as the compensation for a cost incurred by the farmer. We meet the concept first in *Fermiers*, where the following item is included in a list of the farmer's expenses:

> *Pour les intérêts du fonds, des dépenses d'achat de chevaux, charrues, charrettes et autres avances foncières qui périssent, lesquelles, distraction faite de bestiaux, peuvent être estimées 3000 livres, les intérêts sont au moins ....300 livres.*[2]

The concept was further clarified as time went on, but this association of " *intérêt* " with " *avances qui périssent* " was fairly consistently maintained. In Quesnay's description in *Grains* of the " *état de la petite culture des grains* ", the proprietor who supplies oxen to the *métayer* is assumed to be entitled to collect interest at 10 per cent. on the price of the oxen, " *à cause des risques et de la perte sur la vente de ces boeufs, quand ils sont vieux et maigres* ".[3] In the early formulations of the *Tableau*, the " *intérêt* " is analysed separately in order to avoid undue complications,[4] but Quesnay makes it clear that the reason why the " *intérêt* " on the *avances primitives* should amount to at least 10 per cent. is that " *les produits de l'agriculture sont exposés à des*

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[1] The reference to the division of the *produit net* does not occur in Quesnay's manuscript version in the *Archives Nationales*; and the version appearing in *Physiocratie*, significantly enough, divides the *produit net* among the *dixme*, the *impôt*, and the proprietor, making no reference to the farmer (see Oncken, p. 341).

[2] Oncken, pp. 177–8.

[3] *Ibid.*, p. 201.

[4] B.E.A. reprint, p. iii.

accidens ruineux qui, en dix ans, enlèvent au moins la valeur de la récolte d'une année ", and that " *ces avances exigent d'ailleurs beaucoup d'entretien & de renouvellemens* ".[1]

The most extended treatment which Quesnay ever gave to this matter is that in the *Analyse*. The *intérêt* on the *avances primitives* is no longer analysed separately but is formally included in the *reprises* shown in the *Tableau*; and Quesnay gives the following explanation of " *la nécessité de ces intérêts* ":

> *1° Le fonds des richesses d'exploitation qui constituent les avances primitives est sujet à un dépérissement journalier qui exige des réparations continuelles, indispensablement nécessaires pour que ce fonds important reste dans le même état et ne marche pas progressivement vers un anéantissement total qui détruirait la culture et par conséquent la reproduction et par conséquent les richesses de l'Etat et par conséquent aussi la population.*
>
> *2° La culture est inséparable de plusiers grands accidents qui détruisent quelquefois presque entièrement la récolte; telles sont la gelée, la grêle, la nielle, les inondations, la mortalité des bestiaux, etc., etc. Si les cultivateurs n'avaient aucun fonds en réserve, il s'ensuivrait qu'après de tels accidents ils ne pourraient pas payer les propriétaires et le souverain, ou qu'ils ne pourraient pas subvenir aux dépenses de leur culture l'année suivante . . .*
>
> *Les intérêts des avances de l'établissement des cultivateurs doivent donc être compris dans leurs reprises annuelles. Ils servent à faire face à ces grands accidents et à l'entretien journalier des richesses d'exploitation, qui demandent à être réparée sans cesse.*[2]

In the light of this very explicit statement, which was repeated with an equal lack of ambiguity by Mirabeau,[3] the second possibility must be ruled out of court. It should be noted, however, that immediately following the passage just quoted Quesnay went on to develop his argument in a way which left a loophole for later Physiocrats who were anxious to include a net element in the farmer's *intérêt*. This is a point which can most suitably be dealt with in the next section of this article.

Before considering the third and fourth possibilities, let us examine briefly the contexts in which Quesnay speaks specifically of the agricultural entrepreneur's profit and the circumstances in which it will accrue to him. Three more or less separate cases can be distinguished:

1. Entrepreneurs who operate *la grande culture* will normally earn higher profits than those who operate *la petite culture*. " *Il y a . . . plus de profit* ", says Quesnay, " *pour le fermier qui cultive avec des*

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[1] *Ibid.*, p. vii. Quesnay also adds, without any explanation at all, *intérêt* at 10 per cent. on the *avances annuelles*. This he does not do in any of his subsequent versions of the *Tableau*: although the *intérêt* is sometimes stated to be on the *avances primitives et annuelles* (see, e.g., Oncken, pp. 319, 320 and 496) it is always calculated so as to amount *in toto* to 10 per cent. of the *avances primitives*. Mirabeau, on the other hand, usually includes both *intérêt* at 10 per cent. on the *avances primitives* and *intérêt* at 10 per cent. on the *avances annuelles* in his calculations (see, e.g., *Philosophie Rurale* (1764), vol. I, pp. 71 and 325).

[2] Oncken, p. 313.

[3] *Philosophie Rurale*, vol. II, pp. 90–91. Cf. also Baudeau, in Daire, *Physiocrates*, vol. II, p. 825.

chevaux que pour le métayer qui cultive avec des boeufs ".[1] This is the basic theme of *Fermiers*, and it is continued in *Grains*. " *Il est vrai*," writes Quesnay in the latter work, " *qu'un fermier borné à l'emploi d'une charrue ne peut prétendre à un gain considérable ; il n'y a que ceux qui sont assez riches pour se former de plus grands établissements, qui puissent retirer un bon profit et mettre par les dépenses qu'ils peuvent faire, les terres dans la meilleure valeur* ".[2]

2. Entrepreneurs will profit if the price of their product rises (as a result, for example, of the establishment of free trade in corn) during the course of their leases. The ultimate benefit of this rise in price will normally accrue to the proprietors in the form of higher rents,[3] but the short term benefits for the farmer will often be considerable and important:

> *D'abord les fermiers des biens-fonds profitent jusqu'au renouvellement de leurs baux, de l'augmentation constante des prix des productions qui arrive pendant le cours de ces baux. Et ce gain est le plus fructueux, le plus profitable, le plus nécessaire à une nation dont l'agriculture a besoin d'être étendue et améliorée. Car les fermiers, s'ils ne sont pas opprimés, ne quittent point leur état; les profits qu'ils font accroissent leurs richesses d'exploitation, au grand avantage de l'agriculture. Et ces profits qui multiplient les riches fermiers, mettent, lors du renouvellement des baux, une plus grande concurrence entre eux, ce qui assure alors aux propriétaires et au souverain la rentrée entière du produit net, et non seulement de celui qui résulte directement de l'augmentation des prix, mais encore de celui que fait naître en outre la plus grande aisance des fermiers; car on sait que les richesses sont le grand et le principal outil de la culture, et qu'un fermier riche peut souvent louer les terres avec profit à un tiers ou moitié en sus du prix, qu'un fermier pauvre ne pourrait donner que difficilement et au risque de se ruiner.*[4]

3. Entrepreneurs will profit, again during the course of their leases, if they manage to reduce the costs of cultivation. This is illustrated by the remark quoted towards the bottom of page forty-two above, and by a further comment of Quesnay's to the effect that:

> *Le produit du travail de l'artisan ne vaut que la dépense; s'il coûtait plus, il y aurait de la perte. Le produit du travail du cultivateur surpasse la dépense; plus il la surpasse, plus il est profitable, et plus il augmente l'opulence de la nation.*[5]

Now these three types of profit will persist only so long as *la grande culture* is not universal (case 1) or only up to the renewal of the farmers' leases (cases 2 and 3). Thus if we construct a model of the economy in which *la grande culture* is assumed to be universal, and which is sufficiently " long-period " to enable us to assume that any temporary " above-average " profits have crystallised out into rents, we can plausibly abstract from these three types of profit in our model. This, in my opinion, is precisely what Quesnay did in the *Tableau.* The third possibility mentioned above leads us much further in the direction of the solution of the apparent contradiction than the first and second. But it does not lead us all the way there. There remains the final possibility that some form of " profit " may be included in the *reprises annuelles* along with the ordinary costs of cultivation. Here, in the absence of any very specific statement by Quesnay, we must do a little educated guessing. My own feeling is that Quesnay took over the traditional idea that the entrepreneur normally received a " wage " rather higher than that which was paid to his employees—a " wage " which was included, along with the wages of his employees, in the costs of cultivation, and which was recompensed with the other costs out of the proceeds of the harvest.[1] The next historical step—which Quesnay himself did not take but which some of his Physiocratic followers at least approached—was to recognise that this superior " wage " (together with certain other elements of entrepreneurial income) included a portion which in fact constituted a clear or net surplus and which under competitive conditions tended to bear a more or less regular proportion to the amount of capital employed.

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[1] Oncken, p. 167.

[2] *Ibid.*, p. 217. Cf. Mirabeau, *Philosophie Rurale*, vol. I, p. 34.

[3] *Ibid.*, p. 506.

[4] *Ibid.*, pp. 507–8. Cf. the extended treatment of this point in Mirabeau, *Philosophie Rurale*, vol. II, pp. 315 ff.

[5] *Ibid.*, p. 552.

The question of the place which the " profit " of the industrial and commercial entrepreneur occupied in Quesnay's system is easier to answer. It is not true to say that Quesnay was unaware of the importance of capital investment in manufacture,[2] but we can at least say that he regarded investment in agriculture as far more important. If Quesnay's system is indeed a " capitalist " system, the capitalism appears encased in a sort of feudal framework.[3] Agriculture in the *Tableau* is assumed to be more or less subordinated to the capitalist form of production, and the class of proprietors is assumed to appropriate the *produit net* which it yields; but manufacture is still visualised as being conducted by and large by what we would today call " workers on their own account ". The *valeur vénale* of manufactured commodities, therefore, is regarded as being made up solely of " *la valeur même de la matière première et de la subsistance que l'ouvrier a consommée pendant son travail* ".[4] If the artisan economises on his costs, Quesnay argues, the result will not be increased profits for him, but merely a corresponding reduction in the price he will obtain for his commodities. If the farmer economises on his costs, on the other hand, the result will (eventually) be an increase in the *produit net*, since there are " *beaucoup d'autres causes qui peuvent en soutenir la valeur vénale, nonobstant l'épargne sur les frais de la culture* ".[1] This last remark no doubt begs a whole host of questions, but Quesnay's comparison does at least bring out very forcibly the nature of the model of the industrial sector which he employed. We can hardly blame him for employing it: it provided a pretty fair approximation to the actual state of French industry at the time. And the model was, after all, the common property of most of the economists of the day—including, it should be said, the Adam Smith of the *Glasgow Lectures*.

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[1] The following very interesting statement by Mirabeau (*Philosophie Rurale*, vol. I, pp. 312–3) may perhaps be regarded as a confirmation of this interpretation: " *Dans le détail des reprises du Fermier, nous n'avons parlé que de la réproduction de ses avances annuelles & des intérêts, sans avoir rien dit encore de la rétribution due aux soins, aux travaux & aux risques de son entreprise ; parce que cette rétribution se trouve confondue dans les dépenses de ses avances annuelles & dans le produit des bestiaux de profit, qui lui rend les frais de la nourriture, en avoine, de ses chevaux de labour, sur le pied de 600 liv. par charrue. Cette déduction de frais d'exploitation, qui entreroient en compte dans la dépense de ses avances annuelles, n'y sera point comprise. Ainsi elle revient au profit du Laboureur, sur la dépense de ses avances, & ce dédommagement de dépense lui sera alloué pour la rétribution due à son emploi personnel* ".

[2] Cf. Oncken, p. 480, final paragraph.

[3] Cf. Marx, *Theories of Surplus Value* (Lawrence & Wishart edn.), p. 50.

[4] Oncken, p. 537.

Quesnay was of course quite aware of the fact that under certain circumstances those engaged in non-agricultural employments might receive more than a subsistence income. This he explained primarily in terms of the existence of some form or another of monopoly. " *Les ouvrages des artisans à qui le gouvernement accorde des privilèges exclusifs* ", and " *les tableaux des grands peintres et tous les autres ouvrages des artistes qui excellent dans leur profession* ", he admitted, will often sell at prices which " *surpassent de beaucoup la valeur des frais* ", since the workers concerned are so few that " *leur concurrence ne les force pas à mettre leur travail au rabais au profit de ceux qui achètent leurs ouvrages* ".[2] In another place, much less convincingly, Quesnay tried to explain " *les grosses fortunes* " acquired by " *grandes entreprises de commerce* " and " *entreprises de grands travaux* " as a sort of gathering together or accumulation of " *des gains semblables à ceux des petits marchands* " and " *les petits profits que l'on retire du travail d'un grand nombre d'ouvriers* ".[3] This is a poor argument, but it is quite understandable that the existence of such net gains should not have worried the Physiocrats unduly at this early stage in the development of their doctrine. Not only were such gains quantitatively insignificant compared with those yielded in agriculture, but, in addition, once the primacy of agriculture and the expenditure of the class of proprietors was granted, any net gains could reasonably be explained as being in some significant sense " paid out of " the *produit net*—a sort of " *produit net à rebours* ", as Weulersse describes it.[4]

To sum up Quesnay's views on this subject, then, one can probably say that he recognised the existence of entrepreneurial profits, both in agriculture and in manufacture, but reconciled it with his doctrine that land rent was the sole income in the nature of a surplus by:

(*a*) including in the *reprises* of the agricultural entrepreneur a sort of superior " wage " for what Mirabeau called his " *soins, travaux et risques de son entreprise* ";

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[1] *Ibid*., p. 552.

[2] *Ibid*., p. 534. The sentence which follows this is an interesting anticipation of an important aspect of Smithian value theory.

[3] *Ibid*., p. 234. [4] *Op. cit*., vol. I, p. 290.

(*b*) treating any agricultural " profit " over and above this " normal " amount as essentially temporary in character, and therefore assuming that it could properly be abstracted from in the long-period analysis of an economy in which *la bonne culture* was universal; and

(*c*) using a model of the industrial sector which assumed that industry was carried on by workers on their own account, so that any " profit " which in fact emerged could be regarded either as the result of monopoly or as a "*produit net à rebours*".

Thus it can hardly be said that Quesnay himself, as distinct from his disciples, contributed very much *directly* to the formation of the Smithian idea that the supply price of a commodity normally included a " neat or clear " profit which bore a regular proportion to the capital employed.

III

Quesnay's view of profits, then, was rather more sensible and consistent than is commonly appreciated, given the assumptions of the *Tableau* and the particular model of the economy which he adopted. But the contradiction of which I have spoken above remained latent in his system, and in the course of the development of Physiocracy it gradually emerged to the surface—as a result partly of the general march of economic events in the latter half of the eighteenth century, partly of the putting into effect of some of the very reforms which Quesnay himself had advocated, and partly of the cogent criticisms put forward by the Physiocrats' opponents. One can put the essential point simply, if perhaps too mechanically, as follows: With the rise of Smith's third " constituent order " (the capitalist class), and the gradual subordination of both agriculture and manufacture to capitalist methods of production, there is an appreciable increase in the accumulation of new capital out of profits, and in the mobility of capital between different employments. This means that economists become obliged to recognise (*a*) that in the normal case, even under conditions of free competition, the " profit " of the entrepreneur includes a net element which is not merely a compensation for costs; and (*b*) that the man who employs capital in one occupation will normally receive roughly the same rate of net reward as the man who employs it in any other. But if these facts are to be recognised and embodied in the theoretical model one employs to analyse the economy, how can this be done within the framework of a system, like that of Quesnay, which assumes that land rent is the only income in the nature of a surplus and that non-agricultural occupations are " sterile " ? This was the basic difficulty which the later Physiocrats had to face.

As I have said, Quesnay in one place left a loophole for his followers. Continuing the remarks on the farmer's *intérêt* in the *Analyse*, he says:

> *Nous disons d'ailleurs qu'un fonds placé aussi avantageusement pour la nation que celui des avances de sa culture, doit par lui-même rapporter net aux fermiers, qui y joignent leurs travaux et l'emploi de leur intelligence,*

> *un intérêt annuel au moins aussi fort que celui que l'on paye aux rentiers fainéants.*
>
> *La somme totale de ces intérêts se dépense annuellement, parce que les cultivateurs ne les laissent point oisifs; car, dans les intervalles où ils ne sont pas obligés de les employer aux réparations, ils ne manquent pas de les mettre à profit pour accroître et améliorer leur culture, sans quoi ils en pourraient pas subvenir aux grands accidents. Voilà pourquoi on compte les intérêts dans la somme des dépenses annuelles.*[1]

Quesnay's mode of expression here is typically imprecise, but there is at least a hint that the *intérêt* may possibly contain an element that is truly net and which may under certain circumstances be available for accumulation. Even before the *Analyse* appeared, Du Pont had attempted to justify a 10 per cent. rate of interest not only by reason of the high rate of loss and depreciation to which the farmer's capital was subject, but also by reason of the fact that "*tout travail mérite récompense, et . . . il ne serait ni juste, ni sûr que celui qui est le plus pénible, et de qui dépendent tous les autres, fût privé de la chose qu'ils prétendent tous*".[2] And Quesnay's significant comparison between the *intérêt* of the farmers and that of the "*rentiers fainéants*" must have been a direct incitement to revisionism. The Comte d'Albon, indeed, writing in 1775, shortly after Quesnay's death, went so far as to attribute to him the view that the returns must not only replace the *avances annuelles* and provide for the maintenance intact of the *avances primitives*, but must also yield "*une sorte d'intérêt pour les capitaux qu'on a employé à ces avances: de sorte que la profession du cultivateur ne soit pas moins profitable à celui qui l'exerce que toute autre profession n'aurait pu l'être*".[3] I do not think Quesnay himself did actually hold this view, but the fact that Albon could attribute it to him in the *Nouvelles Ephémérides* is an interesting indication of the extent to which the ideas of the School had been developed during the years immediately preceding Quesnay's death.

Mercier de la Rivière, in his *L'Ordre Naturel et Essentiel des Sociétés Politiques* (1767), made at least three amendments to the Physiocratic canon as Quesnay had presented it. First, he emphasised that the farmer must receive a net reward *at least* (Mercier's italics) as great as that which he would get if he employed his capital in another profession.[4] Second, in his analysis of the constituents of what he called the *prix nécessaire* (i.e., supply price) of a manufactured commodity, he included not only the cost of the raw material and the expenses of the artisan and the merchant, but also "*les intérêts qu'il* [the merchant] *doit retirer de* [*ses*] *avances; . . . les rétributions dues aux peines qu'il se donne personnellement; . . . la valeur des différents risques auxquels son commerce l'expose*".[5] Third, in connection with this last item, he affirmed that " *ces risques cependant ne se réalisent pas toujours également pour tous les marchands, & de la différence qui se trouve dans ces accidents, doit naître une différence dans leurs profits* ".[1] Mercier has obviously travelled some distance towards the Smithian concept of profit, but he has not yet arrived at it. Much more radical alterations were made by Baudeau and Turgot, who were the main agents in the reform of Physiocracy.

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[1] Oncken, p. 314.

[2] *De l'Exportation et de l'Inportation des Grains* (1764), pp. 3–4. Quotation from the 1911 reprint (ed. E. Depitre), p. 2.

[3] Oncken, p. 55. Cf. the radically different interpretation of Quesnay's *intérêt* doctrine given in an *Eloge* by the Marquis de Mesmon (Oncken, p. 102).

[4] *L'Ordre Naturel*, p. 223. [5] *Ibid.*, pp. 428–9.

Baudeau's chief contribution was his firm recognition of the importance of the class division between entrepreneurs and wage earners. He still maintains the original Physiocratic distinction between the productive class and the sterile class, but he emphasises, in a way which Quesnay never did, the economic significance of the division of each of these classes between those who " *dirigent et ordonnent les travaux* " and those who " *les font sous leurs ordres* ".[2] This division, he makes clear, is just as important in manufacture as it is in agriculture.[3] He waxes lyrical over the great benefits which accrue to society when there emerges " *un chef qui sait, qui veut et qui peut opérer en grand* ",[4] and calls, much more specifically than most of his predecessors, for the application of capital and capitalist methods to manufacture.[5] In addition, he emphasises that the returns received by the agricultural entrepreneur must include not only a refund of paid-out costs of cultivation and an amount for the upkeep and repair of the *avances primitives*, but also in addition " *une juste compensation des avances, des peines et des dangers ; car on ne peut pas espérer qu'une classe nombreuse, riche et instruite, avance des capitaux considérables, se donne beaucoup de soins continuels, et s'expose à de grands risques, sans retirer cette juste compensation* ".[6] Baudeau also makes it fairly clear that he visualises the income of the industrial entrepreneur as including a similar element;[7] and even in the *produit net* there are two portions which should be exempt from the *impôt*—the just interest on the money which the proprietor has already spent either in putting his land in a cultivable state or in purchasing it, and a fund to cover the amounts which he customarily spends in maintenance and repair.[8] Finally, although Baudeau still uses the word " sterile " to describe the class engaged in non-agricultural pursuits, the distinction between

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[1] *Ibid.*, p. 431.

[2] *Première Introduction à la Philosophie Economique* (1910 edn., ed. A. Dubois), p. 47.

[3] Cf. e.g., *ibid.*, pp. 66–7.

[4] *Ibid.*, p. 67.

[5] Cf., e.g., *ibid.*, p. 68 : " *L'établissement des grands et forts atteliers sous la direction de chefs opulents et industrieux tend donc à procurer au même prix une plus grande somme de jouissances plus agréables* ".

[6] *Ibid.*, p. 116.

[7] Cf., e.g., *ibid.*, p. 67.

[8] *Explication sur le Vrai Sens du Mot Stérile*, reprinted in Daire, *Physiocrates*, vol. II, at p. 871. This recognition of the existence of " privileged " elements in the *produit net* reflects a realisation of the fact that a new type of landed proprietor was coming into existence, and that investment of capital in the ownership of land was now coming to be regarded as just as normal as investment in its cultivation.

" productive " and " sterile " is virtually attenuated by him into a purely chronological one.[1]

Turgot's brilliant analysis of the employment of capital in industry and agriculture opened up even wider possibilities. Capital, writes Turgot, can be employed in five different ways—in purchasing a landed estate; in taking a lease of an estate and cultivating it as a tenant-farmer; in investment in industry; in investment in commerce; or in making loans at interest. The first type of investment yields the least revenue, because if the farmer is solvent there is little risk and the collection of the revenue involves little trouble. Making loans at interest usually brings in a rather larger reward than investment in a landed estate, since the debtor may become insolvent and therefore the owner of the capital will not " content himself with an interest equal to the revenue of the land which he might buy with the same capital ". Money invested in farming, commerce and industry, however, must necessarily yield a higher rate of return than in the other two fields, since the entrepreneur must " draw a profit to recompense him for his care, his labour, his talents and his risks, and to furnish him in addition with that wherewith he may replace the annual wear and tear of his advances ". What Turgot calls " a sort of equilibrium " establishes itself between all these different employments of capital, since funds will flow towards the most lucrative field of investment, and this " necessarily alters in each of these employments the relation between the capital and the annual produce ".[2] This analysis obviously makes considerable inroads into the doctrine of the sterility of manufacture (Turgot preferred to call industrial workers " stipendiaries "), and is very reminiscent of Adam Smith's account of the consequences of capital mobility in the *Wealth of Nations*.

Inevitably, the views expressed by men like Baudeau and Turgot were grafted on to the body of Physiocracy, and eventually ascribed to Quesnay himself. Du Pont, surveying in the days of his old age the rise of Classical economics, saw clearly that it was Quesnay's emphasis on capital accumulation which constituted, historically speaking, the most important feature of his work. In his extraordinary *Nécrologue* of Quesnay's grandson, written in 1805, Du Pont purports to give a summary of the views of the founder of Physiocracy, but lays hardly any stress on the doctrine of sterility or the notion of the dependence of manufacture upon agriculture. He takes as his theme—or rather as Quesnay's theme—the maxim " *respectez donc les capitaux* ". " *L'augmentation des capitaux*," he writes, " *est donc le principal moyen d'accroître le travail, et le plus grand intérêt de la société*." Land *and capital* are the sources of wealth. Agriculture is still vaguely conceived as the only " productive " occupation, but respect is due to *all* capitals —and to all capitalists—whether in agriculture or in any other sphere.

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[1] *Explication*, pp. 868ff.

[2] *Reflections on the Formation and the Distribution of Riches* (Ashley edn. of 1898), pp. 81–5.

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Summing up what he now believes to have been the essence of Quesnay's doctrine, Du Pont writes: "*Le respect pour la liberté et la propriété exige que les hommes et les capitalistes demeurent complètement des maîtres à l'usage de leurs avances et de leur temps, pourvu qu'il n'en résulte ni gêne à la liberté, ni dommage à la propriété de personne*". The *impôt* should never be permitted to encroach upon "*les avances que les travaux exigent, et qui doivent être remboursées avec profit aux entrepreneurs, si l'on ne veut pas tarir ou restreindre la source du travail et des richesses*".[1] One is irresistibly reminded of the way in which certain of the followers of Ricardo began their discussion of value by affirming that Ricardo had said the last word on this subject, and then calmly proceeded to modify his doctrine out of all recognition.

But the framework of traditional Physiocratic theory was too rigid, too inseparably connected with the sort of feudal capitalism which Quesnay and Mirabeau took as their ideal, to contain these new ideas for long. Even in the work of Turgot, who was more a fellow-traveller of the Physiocrats than a Physiocrat *pur*, the difficulties are evident. The main problem which has to be faced, of course, is the reconciliation of the idea that the agricultural *produit net* is the only true form of surplus with the manifest fact that anyone who employs capital, in whatever occupation, normally receives a reward which at least appears to contain an element of true surplus, and out of which new capital can be accumulated. Turgot was quite unequivocal in his recognition of this fact. Entrepreneurs, he says, normally receive profits proportionate to their advances, their talent, and their activity, and have a surplus over and above their subsistence which they save in order to plough it back into their enterprises.[2] Should not these profits, then, be put on a par with the revenue received by the proprietors of the land? Turgot answers in the negative, but is obviously rather hard put to it to defend his answer. There are two main lines of defence which he adopts. First, he argues, even though new capitals are indeed formed by saving out of profits, these profits always come ultimately from the land, since they are all paid "either from the revenue, or as part of the expenditure which serves to produce the revenue". They are in effect only "the accumulation of the part of the values produced by the land that the proprietors of the revenue, or those who share it with them, can lay by every year without using it for the satisfaction of their wants.[3] Second, an important part of these profits is merely the compensation for a sort of opportunity cost: the returns on capital in any employment must be sufficient to yield an income at least as great as that which can be earned in any other employment, since otherwise the capital will be transferred to that other employment. Interest on money lent may appear at first sight to be truly *disponible*, Turgot argues, but it is not really so: if the state tried to appropriate it by means of taxation, the lender would simply withdraw his money.[1] The agricultural entrepreneurs must receive a return sufficient to pay " a profit equal to the revenue they could acquire with their capitals without labour ", or they will not employ their wealth and their toil to cultivate another man's land.[2] And the industrial entrepreneur must receive a profit sufficient to compensate him " for what his money would have been worth to him if he had employed it in the purchase of an estate ".[3]

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[1] The *Nécrologue* is reprinted in Oncken, pp. 801ff. Another example of the tendency discussed in this paragraph is to be found in Du Pont's note to Turgot's *Eloge de Gournay*, reprinted in Daire's edn. of Turgot's *Oeuvres*, pp. 266–8.

[2] *Reflections*, p. 97.

[3] *Ibid.*, p. 98.

But both lines of defence are purely formal. The first in effect assumes what has to be proved; the second, however useful it may be in an analysis of general equilibrium, says merely that capital has a supply price. It is fairly evident, on Turgot's own showing, that profit on capital does in fact include an element which is *disponible* in the original Physiocratic sense of that word—a point which Hume, in a little-known letter to Turgot, was quick to seize upon.[4] With Turgot, Physiocracy begins to burst its seams: the framework of concepts elaborated by Quesnay can no longer accommodate the analysis of the basic economic phenomena of capitalist society. Turgot, however, went much further in preparing the way for Adam Smith than any of his predecessors or contemporaries: with him, the *prix nécessaire* came to include, as it was to do with Smith, a " normal " return to capital, part of which was available for accumulation. It remained only to remove the Physiocratic integument, and the way was clear for the emergence of the *Wealth of Nations*.

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[1] *Ibid.*, pp. 92–3.
[2] *Ibid.*, pp. 55–6.
[3] *Ibid.*, p. 53.
[4] *The Letters of David Hume*, ed. J. Y. T. Greig, vol. II, p. 94.

Abraham Darby and the Coke-smelting Process

By M. W. Flinn

Before the publication of Professor Ashton's *Iron and Steel in the Industrial Revolution* in 1924, it was believed that the coke-smelting process was first used at Coalbrookdale either in 1713[1] or 1730–35.[2] A study of the records of the Coalbrookdale Company enabled Professor Ashton to show that the invention must be credited to Abraham Darby I, who died in 1717, and that all the evidence pointed to the year 1709 as the date of its first use. This opinion has since been confirmed by Dr. Raistrick.[3] More recently, Dr. R. A. Mott, who successfully deflated the claims of Dud Dudley over a score of years ago, has thrown in the weight of a scientist's opinion in favour of the year 1709.[4] This concensus of authoritative opinion naturally leaves very little room for doubt, yet it is founded on a strange persistence in ignoring certain contradictory evidence.

The evidence in favour of the year 1709 has been drawn from three contemporary sources—the furnace accounts at Coalbrookdale for the year 1709, a letter from Abraham Darby I to William Rawlinson dated July 12, 1712,[5] and an account of Abraham Darby's early activities at Coalbrookdale in a letter written by Abiah Darby in 1775.[6] It is the detail of the first of these which has been used principally to determine the date of the invention. The evidence hinges to some extent on the interpretation of the words *coles* and *coling* which occur frequently in the furnace accounts for 1709. Because the *coles* used by Darby in his furnace were supplied partly by Richard Hartshorn, a man who possessed coalmines as well as iron mines, it has been assumed that these *coles* were mineral fuel. But *coles* at this period normally meant *charcoal*.[7] If mineral fuel had been intended, contemporary writers would almost certainly have used the expressions

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[1] H. Scrivenor, *History of the Iron Trade* (2nd ed., 1854), pp. 55–56.

[2] J. Percy, *Metallurgy* (1863), p. 888.

[3] A. Raistrick, *Dynasty of Ironfounders* (1953), pp. 37–40.

[4] R. A. Mott, " The Earliest Use of Coke for Iron Making," *Gas World* (Coking Section), January 5, 1957, pp. 7–18; " Abraham Darby (I and II) and the coal-iron industry," paper read to the Newcomen Society, December 11, 1957.

[5] W. H. Chaloner, " Further Light on the Invention of the Process for Smelting Iron Ore with Coke," *Econ. Hist. Rev.*, 2nd ser. II (1949), pp. 185–87.

[6] Reproduced in full in T. S. Ashton, *Iron and Steel*, pp. 249–52.

[7] For example, in the exclusively charcoal iron district of Furness in the early 18th century, the word *coals* was almost invariably used to mean charcoal—" For fetching Coals out of woodland in night to keep the Furnace going," " to row the Company's coals upon Windermere to our landing nigh Newby Bridge " (A. Fell, *The Early Iron Industry of Furness* (Ulverstôn, 1908), pp. 136, 137). In that district, too, the expression *colepittes* was used for the holes in the ground which formed the bases of the circular piles of wood to be burnt into charcoal (Fell, p. 133). John Ray, in his well-known description of ironmaking of 1674, uses the word *coles* in a context which can only refer to charcoal—" When the mine is brought in, they

pit-coal, *stone-coal* or *sea-coal*.[1] Further, the Coalbrookdale accounts show that on August 24, 1709, Edward Dorrall was paid for *coling coles*, and Professor Ashton suggests in parentheses that *coking* was intended here.[2] But *coling* was the common expression for the making of charcoal, and is perfectly logical in this context, if *coles* mean *charcoal*.[3] It should be noticed that Abiah Darby, in her account of 1775, was most careful to distinguish between wood *charcoal* and *pit coal*. Nevertheless, by interpreting *coles* as mineral fuel, Dr. Raistrick is satisfied that " charcoal is nowhere mentioned in the account of the first blast ",[4] and Professor Ashton concludes that " the paucity of entries relating to the purchase of charcoal implies that the bulk of the fuel used was of mineral origin ".[5] It is possible to claim that these journal entries can relate to mineral fuel, but in view of the contemporary connotation in the iron industry of the words *coles*, *coling* and *charking*, the entries are more likely to indicate the use of charcoal. Other evidence supports the view that the fuel is unlikely to have been coal or coke as early as 1709.

Turning to Abiah Darby's letter of 1775,[6] it will be clear that she distinguished three separate phases in the development of blast furnace

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take small cole, and lay a row of small cole and upon it a row of mine . . . ," " Of 24 loads of coals they expect 8 tun of sow " (John Ray, *A Collection of English Words not generally used* (1674), pp. 125, 127). The same meaning can only be attached to Ambrose Crowley's use of the word in a questionnaire given to his half-brother in 1701 relating to the exclusively charcoal Swedish iron industry. " What quantity of Coales each measure of Wood will make," " What they give for Coaleing their Measures," " If they use a different sort of Coals at the Furnace from what they use at the Forge . . ." (MS. in possession of Mr. H. Lloyd, Marlow, Bucks).

[1] For example, at Redbrooke furnace in the Forest of Dean, where experiments with coke-smelting were made in 1716–17, the iron was " made Wth Stone Cole . . . " (B. L. C. Johnson, " New Light on the Iron Industry of the Forest of Dean," *Trans. Bristol & Glos. Arch. Soc.*, Vol. 72 (1953), 137). When the Carron ironworks were being set up in 1759, Dr. Roebuck engaged " a clever founder or furnaceman who was used to working with pit coal as well as wood fuel " (H. Hamilton, " The Founding of Carron Ironworks ", *Scottish Hist. Rev.*, Vol. 25, (1927–28), p. 190.

[2] Ashton, *op. cit.*, p. 30.

[3] Similarly, the payment on April 20, 1709, for *charking coles* almost certainly refers to the making of charcoal. In Crowley's questionnaire already quoted, there occurs the phrase " Their manner of Coaling, whether they chark it near the places of its Growth, or Carry it to fixed Places of Charking it ". There can, however, be no certainty about the use of the expression *charking*. Originally, it referred specifically to the making of charcoal, and was therefore synonymous with *coling*; later its meaning was extended to cover the making of coke, on account of the similarity of the process. (See *O.E.D.* under " chark ").

[4] Raistrick, *op. cit.*, p. 36.

[5] Ashton, *op. cit.*, p. 30.

[6] Because Abiah's letter does not fit his thesis, Dr. Mott finds it necessary to question the reliability of her evidence, " written when she was of advanced age ". An editorial comment in the *Gas World*, supporting Dr. Mott's argument, refers to a " fairy-tale that arose through the failing memory of an aged lady ". This will not do, for Abiah was born in 1716, and she was therefore 59 at the time of writing the letter. Raistrick described her as a very serious-minded woman. The source of Abiah's information, in her own words, was " what I have heard my husband [Abraham Darby II] say, and what arises from my own knowledge; also what I am informed from a person now living, whose father came here as a workman at the first beginning of these Pit Coal Works " (Ashton, *op. cit.*, p. 249).

technique at Coalbrookdale. In the first phase, Darby " cast iron Goods in sand out of the Blast Furnace that blow'd with wood charcoal; *for it was not yet thought of to blow with Pit Coal*"; in the second, he " try'd with raw coal as it came out of the Mines, but it did not answer"; in the third, he " had the coal coak'd into Cynder, as is done for drying Malt, and it then succeeded to his satisfaction".[1] Darby acquired the Coalbrookdale furnace in 1708, and it was brought into blast for the first time by him in January, 1709.[2] The entries of payments for *coles* which have been interpreted to indicate that mineral fuel was being used in the furnace relate to July, 1709. Is it possible that the three stages indicated by Abiah had been passed through in six months? This seems unlikely, in view of her remark that it was only *sometime after*[3] the initiation of the blast with charcoal that Darby " suggested the thought, that it might be practable to smelt the Iron from the ore in the blast Furnace with Pit Coal".[4] Abiah had no axe to grind, nor any reason for presenting to her correspondent an account that was anything but as accurate as she could give. Samuel Smiles, who had access to the " Blast Furnace Memorandum Book" of Abraham Darby I, now no longer to be found, and who, like Abiah Darby, had no reason to falsify history, showed that the " fuel used in the furnaces appears . . . to have been at first entirely charcoal; but the growing scarcity of wood seems to have gradually led to the use of coke . . ."[5] The *gradual* exhaustion of wood in the well-wooded Coalbrookdale area can hardly have taken place in six months.

Dr. Mott's argument in favour of the year 1709 confounds the reader by its monumental scale (each of the two articles quoted above runs to some 12,000 words), and by the intricacy of its detail. However, his material is the same as that available to all other investigators—the surviving records of the Darby firm—but his approach is quite different, being that of the analytical scientist. He begins by satisfying himself that *coles* may be safely interpreted as mineral fuel, though, as has been argued above, other contemporary records of the iron industry do not justify this assumption.[6] He then shows that had Darby used charcoal in 1709, the iron he made would have cost more to produce than he sold it for, indicating that it was unlikely that charcoal was used. This calculation is based, not on Darby's costs in 1709, for which all the relevant figures are not available, but on the cost of cordwood in Coalbrookdale in 1718, the consumption of charcoal per ton of pig iron in Yorkshire in 1742 and on transport costs in Coalbrookdale after 1720. Without going more deeply into the question of general price movements, this is shaky ground on which to build, particularly when it leads to calculations running to three decimal places. There is, moreover, no justification for assuming that the surviving accounts of 1709 are complete, and that the quantities of raw materials and sales recorded there are complete. Perhaps it would be wiser to admit that the Darby account book of 1708–9 does not contain sufficiently detailed information to establish beyond dispute the use of one fuel or another. Dr. Mott makes a great deal of use of the accounts for the period after 1718, when the use of coke is undisputed, in order to illuminate the 1709 account, but this dangerous form of extrapolation is based on innumerable minor assumptions, very few of which would stand careful scrutiny. Analysis of this kind, relevant to 20th-century technology, can at best only be acceptable for the early 18th century when it is corroborated by other documentary evidence: it can never be accepted to the point of overriding conflicting evidence, least of all when the latter is well founded.

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[1] My italics. Referring to this passage, Dr. Raistrick says: " We can now feel more sure that the charcoal smelting was mainly limited to the Bristol period . . ." (*op. cit.*, p. 38). Dr. Raistrick is surely mistaken in assuming that Darby smelted iron in Bristol, for this would have required a blast furnace. Before going to Coalbrookdale, Darby remelted pig iron in a reverberatory furnace for re-casting.

[2] Raistrick, *op. cit.*, p. 34.

[3] In his enthusiasm for an early date, Dr. Raistrick turns this phrase into " *a little later*" (*op. cit.*, p. 38).

[4] Ashton, *op. cit.*, p. 250.

[5] S. Smiles, *Industrial Biography: Ironworkers and Toolmakers* (1908 ed.), p. 83. Dr. Mott suggests that Smiles' " account of Abraham Darby falls below the standard of his very readable accounts of the early engineers", and finding Smiles' verdict inconvenient, dismisses his evidence on the grounds that " he was unable to consult the records which are now available" (*Gas World*, p. 7). Certainly he could not consult all the records now available, but he could consult the all-important Blast Furnace Memorandum Book not available to us, and his evidence drawn from this source therefore assumes considerable importance.

[6] Dr. Mott writes (*Gas World*, p. 9): " Since in Sussex the term 'cole' was used for charcoal, previous observers have hesitated to identify this term positively with

The foregoing suggests that the year 1709 is too early for the first use of the coke-smelting process. What then of Darby's letter of 1712? The letter was written to William Rawlinson, one of the Quaker partners in the Backbarrow Iron Company, immediately after Darby's return from a visit to Rawlinson in July, 1712. In it, Darby spoke of " some other things in vew in youer parts moore then making iron in the common way and with common fuell ", a statement which, on its own, justified Dr. Chaloner's opinion that the letter confirms that the coke-smelting process was invented by 1712. However, careful reading of the whole of the relevant section suggests, in fact, that Darby considered starting some sort of a business *in the Furness district* either alone or in partnership with Rawlinson with the object of initiating some new project.[1] As there was no coal in the Furness area, but plenty of

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mineral coal. This hesitation need not be maintained for in the mineral coalfields the unqualified term ' cole ' meant mineral coal by the end of the 17th century." Darby, however, was an ironmaster, not a coalmaster, and might be expected to use the term with the connotation in the iron industry of charcoal.

[1] The relevant passage reads: " I had some other things in vew in youer parts moore then making iron in the common way and with common fuell, but did partly forget it at coming away, and partly the seson, being first day, was unfit for buisnes of that nature, and, to dell plainly, I had some hesitations about comunicating that new method at this time, but rather did intend to defer it for 2 or 3 years longer, and then thought I might dow something *either with thou and Compa*[*ny*], *or with ower intrest seperatly in youer parts*, for which I had sundry resons, such as *being fixed at this furnes for* 5 *years* and could not well *draw out stock in this place* to put in with you, but must *staie on the work hear*." (Chaloner, *loc. cit*., p. 186. My italics).

charcoal, this can only mean that, whatever the business referred to was, it was not coke-smelting.[1] It would be unwise, therefore, to interpret the letter as confirming that the coke-smelting process was already invented by 1712.

If Smiles' opinion (based, it must be recalled, on an important document no longer available) is taken into account, the adoption of coke fuel in the Coalbrookdale furnace was a gradual process of substitution. There was a period when the fuel used was a mixture of charcoal and coke; but even the first use of this mixed fuel can hardly have occurred as early as 1709. If Abiah's authority is accepted, and there seems no reason why it should not be, the credit for the invention must go to Abraham I; and he died in 1717. Moreover, W. G. Norris' evidence of the use of coal in 1718 confirms this.[2] Thus the only reasonable conclusion is that the process was first used some time between 1709 and 1717, but that it is unlikely to have been used in the early part of that period. Until further evidence comes to hand, it is impossible to be more precise. To name a single year would be guesswork, though clearly we should not go far wrong in accepting the date suggested by Scrivenor over a century ago of 1713.

Why, then, did Darby go to Coalbrookdale in 1708, if not to smelt iron with coal? His familiarity with the use of coke in copper-smelting and malt-making does not in itself justify the assumption that he intended or attempted to smelt iron with coal *from the start*. He wanted to make iron, and the Coalbrookdale furnace was one of very few (if not the only one) that must have been on the market in the year 1708 which was easily accessible to Bristol. Once there, and having satisfied his immediate business aim of producing iron, he was later able to turn his attention to the technical experiments to which his earlier experience prompted him. This is a simple explanation, but one which is not invalidated by any part of the surviving documentary evidence.

Quite apart from the virtue of precision for its own sake in dating the initiation of a most profound change in the structure of the English economy, the chronology of Darby's technical progress, if accurately established, may throw some useful light on the process of invention itself. Before going to Coalbrookdale late in 1708, Darby had no experience of iron-smelting, and the immediate success with coke-smelting early in 1709 postulated by Ashton, Raistrick and Mott implies that his invention resulted from the straightforward application of a simple idea by an " outsider " to a problem which had defied the " insiders " for over a century. On the other hand, a lag of perhaps four or five years between Darby's arrival at Coalbrookdale and his successful use of coke fits more acceptably into the conception of invention as an empirical, evolutionary process. In this context, Darby appears as a practising ironmaster, informed about cognate processes in other industries, patiently adapting techniques and acquiring deeper insight into the peculiar difficulties of iron-smelting, until the process of trial and error led him to a solution.

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[1] One possible explanation of the passage in n.1, p. 57 is that Darby was referring to experiments in smelting with peat, which is known to have been employed successfully as blast furnace fuel in the early 18th century either on its own or mixed with other fuels both at Coalbrookdale and in the Furness district.

[2] Smiles, *op. cit.*, p. 339; Ashton, *op. cit.*, p. 29.

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Methodological Prescriptions in Economics

By K. KLAPPHOLZ AND J. AGASSI

The two books under review[1] offer somewhat extreme examples of an impatience which is often to be found in methodological criticisms of economics. This impatience has a variety of targets, such as the " unnecessarily " slow progress in economics or the futility of much of the work done, and sometimes expresses itself in the complaint that economists often advance hypotheses which appear to be untestable, etc. And the impatience appears to give rise to the belief that, if only economists adopted this or that methodological rule, the road ahead would at least be cleared (and possibly the traffic would move briskly along it).

Our view, on the contrary, is that there is only one generally applicable methodological rule, and that is the exhortation to be critical and always ready to subject one's hypotheses to critical scrutiny. Any attempt to reinforce this general maxim by a set of additional rules is likely to be futile and possibly harmful.

To illustrate our contention we shall examine some of the methodological rules proposed not only in the books under review but also in some other recent writings on the methodology of economics. Our criticism will be based on our methodological point of view, which is that outlined in K. R. Popper's *Logic of Scientific Discovery*.

I

We begin with Lionel Robbins' *Essay on the Nature and Significance of Economic Science* (second edition, 1935), since it is the first significant English work in this field in this century, and some subsequent discussions to which we refer can be viewed as (somewhat excessive) reactions to Robbins' attitude.

Even though Robbins' aim was " not to discover how Economics should be pursued " (p. 72), it is clear that methodological prescription was prominent in his *Essay*. We note, in particular, his suggestion that there should be an *a priori*, water-tight, separation between economics and other sciences. This amounted to the *a priori* decision to view certain variables of economics (e.g. tastes and technology) as essentially " exogenous ", i.e. as not determined within economic models, rather

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[1] Sidney Schoeffler, *The Failures of Economics: a Diagnostic Study*, Harvard University Press, 1955. xv + 254 pp., 38s.

Andreas G. Papandreou, *Economics as a Science*, L. B. Lippincott Company, 1958. x + 146 pp.

We wish to thank Mr. L. P. Foldes and Professor L. Robbins for their valuable criticisms and suggestions.

than as not yet explained by any existing economic model. Robbins did not, of course, object to attempts to explain " tastes " psychologically or sociologically, but according to his view no explanation of them should contain (endogenous) economic variables. Secondly, he denied *a priori* the possibility of discovering " quantitative " laws in economics.

We find these suggestions unacceptable because they are designed to limit the field of argument *a priori*. As a hypothesis, the view that tastes are independent of other economic factors could be subject to critical discussion; it could be countered by an alternative hypothesis describing conceivable relations between income, prices, tastes, etc. Robbins apparently did not advance his view that tastes are exogenous as a hypothesis, but rather as a methodological rule designed to delineate the area of economic discussion. The rule, in its turn, was based on more general considerations—on his general view of economics as a science.

Robbins regarded scientific laws as universal statements known with certainty to be true; laws were statements of the necessary aspects of natural phenomena. Examples of such laws in economics were the law of demand, the law of diminishing returns, and the quantity theory of money. How was knowledge of these laws obtained? It was derivable neither from history nor from controlled experiment, but rather obtained by a process of " deduction from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience. . . . They are so much the stuff of everyday experience that they have only to be stated to be recognised as obvious " (pp. 78, 79).

Everyday experience might perhaps tell us that tastes are exogenous, but it certainly cannot tell us that tastes, or any other factor, must be regarded as exogenous in all future theories. The view, however, that economic laws are certain and must be based on everyday experience, does somehow entail that some factor or other must be exogenous.

The assertion that a law exists which describes the relation between the quantity of money in circulation and the price-level amounts to the claim that only a finite number of specifiable factors are relevant to this relation; for if a statement of this relation is made in which some relevant factors are overlooked, the statement will be refuted by showing the relevance of these factors. For example, a crude version of the theory ignored economic growth, and was thus refutable by showing that economic growth might prevent a rise in the price-level in spite of an increase of the quantity of money.

Thus a law, to be true, must correctly refer to all the relevant factors, which must of course be finite in number (since otherwise we could not mention them all). Now, if one defends a law one also implicitly defends the truth of the assertion that only a finite number of identifiable factors are relevant to the phenomena explained by it. One may, however, invert the procedure by asserting the following: even before we have any particular law we know that we can discover some laws, namely those explaining phenomena, which are subject to a finite number of identifiable factors. This view, when extended to all phenomena, is the view which J. M. Keynes, in his *Treatise on Probability*, called the *principle of limited variety*.[1] This principle amounts to the *a priori* assertion that every effect has a finite number of possible causes which are identifiable *a priori*; it thus implies that *all* laws are discoverable.

Now Robbins, unlike Keynes, did not accept the principle of limited variety as applicable to all cases. For example, he was agnostic as to the existence of quantitative laws, and merely claimed that we could not discover them. It seems, however, that Robbins did claim the following: since everyday experience reveals that certain economic phenomena are governed by a finite number of identifiable factors (since otherwise economic behaviour could not be rational), everyday experience guarantees the existence of laws which explain *some* phenomena. If one insists that only laws guaranteed by everyday experience will be found, one can hardly escape the conclusion that certain variables will inevitably remain exogenous.

One could criticise this position from a logical point of view, by arguing that empirical statements, however well-grounded in everyday experience (or in controlled experiment), are never indisputable; that we can never identify the relevant factors *a priori*; that many statements which were once considered to be quite indisputable were subsequently shown to be false. Yet such an attack, though logically impeccable, would not be damaging to Robbins' position, which he could retain with slight modifications. He could still claim that, although in principle all of economic theory is disputable, no reasonable person would dispute such statements as the law of demand, and that this law, and other similarly trivial statements, can explain much more than meets the eye. (This is essentially Milton Friedman's position, as we argue in section III).

We therefore dissent from Robbins' point of view rather than criticise his particular conclusions. We agree that many scientific statements are accepted without debate, but we hold that such statements are of little scientific interest. Scientific interest is aroused by controversial statements, and by the controversy round them. Undisputed statements are usually the product of qualifications of statements which were once disputed, and the qualifications are accepted as the result of successful criticism. For example, the quantity theory of money was once hotly disputed; by now the accepted version is so well qualified (by conditional clauses regarding liquidity preference, unemployment, economic growth, etc.) that it is hardly of great *theoretical* interest. To regain this interest, the theory would have to explain what we already know by means of a less qualified, or more universal, statement.

Perhaps we should explain the sense in which we use the term " of scientific interest ". Clearly, the law of demand, the quantity theory of money, and similar propositions are of the utmost importance for economic policy. They are comparable, however, not to the laws of physics which are now the subject of controversy, like quantum theory, but rather to Gray's law of conductivity (metals conduct electricity), which is obviously significant technologically. (See below, section III). This law is as undisputed today as the law of demand, and equally manifest in everyday experience. Yet this is precisely why Gray's law is not of intellectual interest in current research in physics. Similarly, the present wide acceptability of the law of demand renders it an inadequate example of the significance of economic science. The significance of economics, as of any other science, lies in its search for new truths.

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[1] Pp. 234 *et seq.*; cf. Ch. XXIII.

II

As we have said, we do not so much criticise Robbins' position from a formal point of view, but rather from our view of what is of scientific interest. An attempt at a formal criticism of Robbins was undertaken by T. W. Hutchison in his influential book, *The Significance and Basic Postulates of Economic Theory* (1938). Hutchison claimed that " propositions of pure theory " and " economic laws ", as stated by Robbins and most other economists, were " tautologies " or " analytic truths ". This appraisal was important since it was coupled with the prescription that scientific economic discussion should be confined to testable statements; for Hutchison has the great merit of having introduced (Popper's) falsifiability criterion into the discussion.

Hutchison's appraisal was based on the fundamental " ' division by dichotomy ' . . . or exhaustive twofold classification of all propositions which have ' scientific sense ' " (p. 26) into statements which are " conceivably falsifiable by observation " or "*forbid* . . . [some] conceivable occurrence ", and statements which are not. The former are empirical statements and the latter logically true statements, or tautologies. All (scientifically meaningful) statements, he contended, fall into either of these two categories.

Hutchison took it for granted that the class of statements which are " conceivably falsifiable by observation " was identical with the class of those which " *forbid* . . . [some] conceivable occurrence "; which is a mistake. One can imagine a factually false statement which is irrefutable. Such a statement would " *forbid* . . . [some] conceivable occurrence ", and yet would not be " conceivably falsifiable by observation ". In this case these two classes of statements would not be identical. Hutchison argued, for example (Ch. II, 5), that since statements with unspecified *ceteris paribus* clauses are irrefutable, they are tautological (or logically true). Yet it can easily be proved that these unfalsifiable statements are not usually tautologies. Consider the two statements: " *ceteris paribus*, the imposition of a tax on cigarettes will raise their price ", and " *ceteris paribus*, the imposition of a tax on cigarettes will *not* raise their price ". These statements are untestable as long as the *ceteris paribus* clauses in them remain unspecified. But they are clearly not both tautologies, for they are incompatible with each other. Yet, according to Hutchison, both are tautologies. Thus Hutchison's identification of " conceivably refutable " with " possibly false " led him to the inconsistency of viewing as tautological all statements of economics which are qualified by unspecified *ceteris paribus* clauses.

To be sure, we do not claim that Hutchison was always mistaken in viewing propositions of pure theory as tautologies. Yet even where he is right, his argument depends on a particular interpretation of statements of pure theory, an interpretation which is neither necessary nor conventional. Often he criticised economists for uttering tautologies, when their utterances were not tautologies except on his interpretation. From his survey of economic theory one gets the impression that most economic theorists of his day uttered almost nothing but tautologies, although his book appeared two years after Keynes' *General Theory*. Yet Keynes was undoubtedly concerned with empirical issues.

We may give an example of Hutchison's imposition of his own meaning on other people's writings. Robbins had claimed that the quantity theory of money was indisputable and had predictive power (p. 117). Had Hutchison criticised this on the grounds that no theory is both indisputable and informative, we would have no comment. Yet we see no reason for his contention that " we find empirical content being read [by Robbins] into propositions of pure theory just because of their necessity and inevitability " (p. 65). The reference to " necessity and inevitability ", which for Robbins was the necessity of a verified empirical law, was by no means sufficient ground for the claim that Robbins had stated the quantity theory of money as a tautology. If we understand him rightly, Hutchison argued (p. 66) that " $MV=PT$ " *is* a tautology, though " $M=kP$ " is not. This invites a few comments. First, since Robbins had referred to Sir William Petty's quantity theory of money, which is nearer to " $M=kP$ " than to " $MV=PT$ ", Hutchison should have noticed that in Robbins' sense the theory was not a tautology, rather than blame Robbins for " reading content " into a tautology. Secondly, Hutchison did not show that " $MV=PT$ " was a tautology in all conventional interpretations. Thirdly, reading empirical content into a tautology is a perfectly respectacle procedure (at least since Hilbert's system of geometry was devised). True, the reading of content into a tautology renders it into a non-tautology, and robs it of its certitude; but it also does give it predictive power in many cases. Quite generally, we have great latitude in the interpretation of postulates. We often have the choice of viewing them either as tautologies or as hypotheses, as Hutchison himself pointed out (pp. 43, 44). Why then did he insist on viewing most of the " propositions of pure theory " as tautologies ? The reason seems to be quite clear: he was driven to do so by the adoption of his dichotomy. He regarded most propositions of " pure theory " as untestable and, given the dichotomy, a statement which appeared to be untestable could be nothing but a tautology.

Hutchison adopted the dichotomy in order to propose the rule, " Do not argue about tautologies, but only about testable statements ! " We have sought to show that Hutchison's dichotomy is false. If we still wished to follow his dictum, we should have to rule out as uninteresting not merely tautologies but also all untestable (or seemingly untestable) statements. This would amount to an undesirable restriction on the range of argument, especially since it is often difficult to know whether an important new idea is testable. It would surely be a mistake to rule such an idea out of court merely on the grounds that it did not appear to be testable.

Hutchison advocated the adoption of his rule in order to ensure adherence to the critical attitude. As we have already said, we do not think that the adoption of such rules necessarily has this result (for a discussion may still be *un*critical even though the rules are adopted). Hutchison was primarily concerned with the status (the testability) of statements, believing, apparently, that the restriction of discussion to testable statements would ensure that these statements would be tested and, if refuted, rejected. He seems to have held this view partly because (on the basis of the dichotomy) he thought that the *only* way to escape refutation of an empirical statement was to turn it into a tautology (p. 43). We have argued that this is not so. Yet it partly explains why Hutchison did not explicitly distinguish between the testability of a statement, and its being on the agenda for testing. Yet this distinction may be crucial. Our interest in testability is due to the fact that we are interested in testing. And our interest in testing stems from the fact that we learn by it. Yet in order to learn it is necessary that the test be such as to expose a hypothesis to the risk of falsification. Tests can easily be performed without any risk of falsification (for example, if we repeatedly " test " an astronomical hypothesis by deducing from it the prediction that the sun will rise tomorrow and testing this prediction); but nothing can be learnt from such testing. Moreover, there may be many testable statements which no one would trouble to test. In fact, neither the testability of a hypothesis nor its testing ensures the practical application of the critical attitude. Professor Popper has strongly emphasised this point, namely, that the critical attitude demands *severe* and *sincere* attempts to *falsify* our views, and he also stated the arguments against the dichotomy.

III

It seems to us that Milton Friedman's position, as outlined in the essay " The Methodology of Positive Economics " in his *Essays in Positive Economics* (1953), is an example of the acceptance of the methodological rules which are part and parcel of the critical approach, yet without the wholehearted adoption of the approach itself.

The central thesis of Friedman's essay is that much criticism of economic theory has been methodologically wrong-headed and misses the target, a thesis he easily establishes. Yet in his rebuttal of these misconceived criticisms, Friedman adopts a position which impedes criticism in general. We shall state three of his points, and then criticise them. (*a*) " A hypothesis can be tested *only* by the conformity of its implications or predictions with observable phenomena " (p. 40, our italics). (*b*) " Great confidence is attached to . . . [a hypothesis] if it has survived many opportunities for contradiction " (p. 9). This is apparently the case regarding " existing relative price theory ", which is therefore " deserving of much confidence " (p. 41). (*c*) A new or rival theory " must have implications susceptible to empirical contradiction " before it can be regarded as interesting and important (p. 38).

(*a*) It is clear that empirical testing is the highest and most desirable form of critical argument concerning a theory; and it is also clear that in the history of economics too little attention has been paid to this form of critical argument. Yet it is a cardinal mistake to lay down the rule that empirical testing against observable phenomena should be the only acceptable method of criticism.[1] It seems important to emphasise this because, perhaps as a reaction against the manifestly insufficient attempts to test economic theories empirically, a mood is developing according to which empirical testing is regarded as the *sine qua non* of critical argument.

The insistence on empirical testing as the only form of acceptable criticism amounts to the assertion that untestable theories cannot be criticised and overthrown, which is historically untrue. For example, it is not clear whether Say's Law is empirically testable; yet in some sense it is considered as overthrown and given up. Again, some Mercantilist theories were abandoned as a result of heuristic criticism, rather than because of their conflict with observed phenomena. Furthermore, if we insist that as yet untestable theories cannot be successfully criticised, we are unlikely to develop testable theories at all. Testable theories are developed by a long process of arduous argument; we do not usually start an argument by producing an empirically testable hypothesis. Friedman tends to dismiss the theories of monopolistic competition and full-cost pricing on the grounds, among others, that they are untestable (p. 38, p. 33, note 25). Perhaps they are; but this is not a reason for dismissing them, but rather a reason for criticising them in an attempt to make them testable. One of the problems which the theory of monopolistic competition was designed to tackle was the problem of " product competition ". It is obviously an important problem, and one which has no place in the competitive model. The theory of monopolistic competition may not solve the problem, and may not even have tackled it sensibly. But at the very least it has drawn attention to the problem and provided a framework within which it can be discussed, which is sufficient reason for not dismissing the theory off-hand (especially as long as no alternative framework for the discussion of the problem is available).

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[1] Since any criticism, other than proof of inconsistency, must somehow pertain to some facts, Friedman's dictum may be easily saved by using " empirical test " in a much wider sense than intended. Yet clearly our examples below are not " empirical tests " in the intended sense. Furthermore, one might criticise an explanation of some economic phenomena by pointing out that it unintentionally contained undesirable psychological implications, while if it were modified no such implications would follow and the explanatory power would remain unimpaired. But such kinds of criticism are hardly tests in the ordinary sense of the word.

There is also an " economic " argument against the insistence on testing as the only acceptable form of criticism. It is often possible to refute a hypothesis merely by a *Gedankenexperiment*, or by pointing out that it conflicts with an accepted hypothesis, or with known facts. For example, Galileo's hypothesis (which Friedman regards as " accepted " (p. 16)) was shown to be unsatisfactory by *Gedankenexperimente* concerning the gravitation of the earth and the moon. If this can be done, it may be a more " economical " method of refutation than empirical testing.

(*b*) There are two different senses in which, according to Friedman, a hypothesis is " acceptable " as a result of " . . . its repeated failure to be contradicted " by experience (p. 23), (although he does not make this distinction). In the first sense he claims that the hypothesis is " acceptable " for technological purposes. In the second, he argues that it ought to be accepted in the sense that we ought to believe in its truth.

The two senses of " acceptability " cannot coincide since we have to disbelieve a refuted theory and yet may accept it for technological purposes (in physics as well as in economic policy). Newton's theory is accepted and widely used in technology because in technology we are not concerned with its (false) view of forces as acting at a distance, or that it is inapplicable to strong gravitational fields. At most a technologist who applies Newton's theory has to know that it is inapplicable where very high velocities are involved. Usually a theory accepted for technological purposes must command wide acceptance within the field of its practical application, for application may be dangerous as long as there is serious doubt concerning it. Yet this wide acceptability relates to highly qualified statements, and the qualifications, which are usually the result of past refutations, serve to delineate sharply the field of practical application.

The second sense is that of credibility, or high degree of belief, or confidence in a scientific hypothesis, which results from a failure of attempts to refute it. We deny that this acceptability is compulsory. The refutation of a hypothesis does indeed compel us to reject it; but the absence of successful refutation of a hypothesis certainly does not prescribe belief or " confidence " in it. Confidence in a hypothesis is purely a matter of subjective estimate, and there are no rules or theorems which relate absence of refutation to desirable confidence in a hypothesis.

This is not the place to discuss this controversial philosophic issue.[1] We mention it in order to substantiate our claim that Friedman, in spite of his avowed intentions, does not wholeheartedly accept the critical approach. He wishes to use the absence of refutation as a credential for a theory, whereas the critical attitude is not particularly concerned with credentials but rather with argument. Friedman regards it as " trivial " that economic theory is open to criticism; it obviously offers a target for criticism " in a trivial sense ", he says, because " any theory is necessarily provisional and subject to change with the advance of knowledge " (p. 41). This indeed may be trivial. It seems to us less trivial to stress the fruitfulness of criticism, or the fact that criticism may lead to the advance of knowledge. Criticism is an attempt to show that existing views require modification; if the criticism is successful it may lead to attempts at modification. And every modification of an existing view is itself an advance of knowledge. For this reason we are less impressed than Friedman by the absence of refutations.[1]

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[1] This issue is discussed in Popper's *Logic of Scientific Discovery*, Ch. X.

(*c*) The doctrine that the absence of refutation imposes a desirable degree of confidence in a hypothesis can be an impediment to critical argument. One way to stimulate argument against a hypothesis is to attempt to produce a rival one. Yet it is difficult to produce a rival hypothesis if it is claimed that existing unrefuted hypotheses must command confidence. If, in addition, one insists that a new rival hypothesis must be testable before it is an interesting subject for argument, the production of new hypotheses is made yet more difficult. Thus the doctrine of desirable confidence allied with the insistence on " testability here and now " may be a hindrance to scientific argument and progress and a weapon for the defence of the scientific *status quo*. It is preferable, we think, to help our critic to formulate his rival hypothesis rather than to discourage him.

It seems clear that Friedman did not intend the consequences which we believe to result from his position. Possibly he overlooked them because of his view that the process of constructing new hypotheses " is a creative act of inspiration . . . " which " . . . must be discussed in psychological, not logical, categories " (p. 43). No doubt he is right. Yet there is also a logical category vitally relevant to the process of constructing new hypotheses, and that is criticism. Criticism fosters the creative inspiration which gives rise to the production of new hypotheses in at least two ways. It inspires new attempts, and it helps to eliminate errors which impede new inspiration. Since Friedman stresses the desirability of developing a more general theory of the allocation of resources than the present one, the purpose of his criticism of the theory of monopolistic competition is presumably to stimulate further attempts. Yet his criticism of the theory of monopolistic competition rests mainly on impatient methodology and is conservative and discouraging rather than stimulating.

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[1] We have said before (p. 65) that one can " test " a theory without exposing it to the risk of refutation, and that this is hardly an interesting or useful method of " testing ". To avoid " testing " of this sort, Friedman holds that a hypothesis is particularly interesting if it " . . . yields . . . predictions about phenomena not yet observed " (p. 7), with which we quite agree. Yet Friedman does not adduce a single example of an economic theory which has been tested in this way. (The examples he mentions on pages 22–23 are not of this type).

IV

Although the preceding comments are occasioned by the two books under review, we must say straightaway that these books fall well below the high standards set by the authors we have considered so far. The two books invite criticism because the methodological faults which we have pointed out are here carried to almost incredible extremes.

Professor Papandreou's *Economics as a Science* conveys the following message: models are untestable unless the parameters in their equations are numerically determined. From this he concludes that all economics so far, including Keynesian models, is untestable and hence unscientific. The view that economics is a science is said to be sheer " folklore " and " myth ". In fact his claim amounts to the assertion that unless we start doing econometrics we shall not engage in scientific activity.

We find it difficult to understand how this central thesis of the book could ever have been advanced. It is all the more puzzling since Papandreou seems to accept (pp. 81, 127) P. A. Samuelson's view that qualitative economics is testable to a certain extent (*Foundations*, pp. 7, 257).[1] The only clue seems to be a remark at the very end of the first (introductory) chapter. " Empirical meaningfulness in the Samuelson sense ", we read (on p. 11), " constitutes a program of research rather than an accomplished fact ". Unfortunately, this statement is not substantiated anywhere in the book.

Yet this very statement, or the phrase " empirical meaningfulness " in it, may provide a clue to our first puzzle: why does Papandreou claim that economic models are untestable unless the parameters in them are numerically determined? Our tentative answer may also explain the purpose of the purely terminological discussion which fills more than half of his volume. Although the author seems quite diffident about the purpose of this lengthy discussion, it seems that he tries to construct a language in which only testable statements are meaningful. In this language expressions with unspecified parameters are not meaningful statements but merely statement-schemata (pp. 117–18).

This attempt may be criticised on at least three grounds. First, some theories, which are meaningful in his language, may be untestable (for example, a theory of an economy with 90 per cent. unemployment), while others, meaningless in his language, are testable (for example, the models which Samuelson discusses). Secondly, there is no point in forcing people to state only testable statements. As we have argued, people's willingness to test hypotheses or to accept refutations will not be increased merely by forcing them to utter only testable statements.

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[1] One of Samuelson's main purposes seems to be the use of this testability to the utmost (*Foundations of Economic Analysis*, p. 284).

Thirdly, Papandreou only provides notations and definitions, but he has not constructed a language. He does not specify rules of statement formation; and he does not provide rules of inference. It must have been a slip of his pen when he writes (p. 128) of " derivation (as defined in chapter 2) ", for neither there nor elsewhere in the book does he define " derivation ".

Since the class notation which Papandreou introduces seems now to be coming into vogue, we should warn the reader lest he be too impressed by it. We are rather indifferent to notation; we merely deny that it necessarily adds to clarity or precision. A case in point is Papandreou's own work which looks very impressive indeed. Yet the bits of logic and mathematics it contains leave much to be desired.[1] On the whole, his nomenclature seems to reduce rather than increase both clarity and precision.

V

The second book under review also advances the thesis that there has been no economic science, but goes further and seeks to demonstrate that economics can never be scientific. Thus Professor Schoeffler's *The Failures of Economics* is not only completely outside the Weber-Robbins tradition in economics, which is shared by all other writers we mention, but advocates, moreover, that economists abandon this tradition.

Our reason for examining Schoeffler's work in some detail is that he provides persuasive rationalisations for widely prevalent but utterly dangerous muddles. In general, one prefers to ignore the confusions which occasionally creep into the literature, hoping that they will quietly lapse into oblivion without doing much harm. But this book is better not ignored. There is another reason for taking Schoeffler to task, namely that he fails to take his immediate predecessors seriously. He accuses economists of not having progressed since Mill (p. 1); we accuse him of regressing to Mill and even to ideas which Hume refuted. The demand for scientific progress may be an excessive demand; but the demand to avoid regression is not unreasonable, since it can be met simply by adhering to the vital tradition of not ignoring one's predecessors.

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[1] The author's main concept is that of " generic element " (i.e. " variable "). It is defined (pp. 60–61) with the help of the concept of " power set ", but as the author admits (footnote 1, p. 61) the definition is quite inadequate for his own purposes. The proper definition is much simpler. The author's demand that the " social space ", as he defines it, should be used to specify the circumstances in which the theory is supposed to apply is *apriorist*. For a theory is a *description* of the " social space " whether or not it agrees with the author's definitions. Apart from definitions, the only part of the mathematical exposition (pp. 39–100) which is of substance is on pp. 87–89. The reader who wishes to understand these three pages should consult Samuelson's *Foundations*, Ch. II, especially p. 10.

Prior to the mathematical exposition there is a discussion (pp. 13–38) of deductive systems. This discussion is mainly concerned with formal (uninterpreted) systems—quite useless for economics, of course—and their subsequent interpretations. There is no discussion of deduction or derivation, not even mention of derivation as a truth-preserving transformation. The author stresses that the formulæ of a formal (uninterpreted) system " mean nothing " (p. 15), that its rules of transformation are " arbitrary within certain [?] bounds " (p. 14) and similar to the rules of chess (p. 16). Yet in his example of a formal (uninterpreted) system he has as a rule of transformation " if P and $P \longrightarrow Q$ are true, then Q is true " (p. 23) ! The author is also confused about matters which are of importance for his discussion, e.g. the distinction between derivation and proof (pp. 17, 23).

All these confusions are particularly unfortunate since the author explicitly recommends his book " as an adjunct in classes in economic theory, economic method, and mathematical economics " (p. ix).

" So far, the most spectacular accomplishment of economics has been its failures . . . ", writes Professor Schoeffler, referring to the deplorable inability of economists to make dependable predictions, for example, as to " where the Federal Reserve index of industrial production will stand six months hence " (p. 4). The purpose of his book is " . . . to inquire into the whys and wherefores of these failures . . . before searching for the appropriate curative treatments " (p. 6).

His diagnosis of the causes of these failures is quite simple. Economists have been looking for economic laws although it is clear that such laws do not exist. In so doing, they have been driven to commit a whole host of methodological " artificialities " which render economics useless for dependable prediction. Economists have failed to realise that their field is fundamentally different from that of the natural sciences, and requires a different methodology.

The following are his suggested remedies. Economists should stop attempting the impossible and give up the harmful pretence of being scientists engaged in the discovery of empirical laws. Instead, they should try to be useful and help to solve practical economic problems. This would indeed require prediction. Since the crux of Schoeffler's thesis is that there can be no economic laws, predictions in economics must be based on facts and on " . . . laws *already established by other sciences* " (p. 157, author's italics), and derived with the aid of inductive logic. The sciences which are to provide economists with laws are the natural sciences as well as " psychology, social psychology, and perhaps, sociology . . . " (p. 158). Professor Schoeffler nowhere explains why the other social sciences should be able to establish laws while economics cannot do so. Nor does he tell us what some of these laws are, except that " there is little doubt " that " dynamic laws of habit and attitude formation and change . . . actually exist " (p. 78). The main fountain of economic predictions, however, should be the rules of inductive logic which apparently enable us to make dependable predictions even without laws, although laws (from *other* sciences) may render the predictions even more dependable.

How does Schoeffler come to make such suggestions? Why does he hold that " the history of economic thought . . . constitutes a gigantic blind alley . . . " (p. 162)? This question leads us to his general view of science.

Professor Schoeffler is an inductivist. He believes implicitly in the possibility of making probabilistic predictions of some future events *solely* on the basis of past information, and that we can predict some future events with certainty on the basis of inductively verified laws. His view of verification closely follows Mill's: a law, he holds, is a statement whose " . . . validity conditions are fully stated; that is, it holds true, as it stands, under all possible surrounding circumstances. There should be no unstated validity conditions a change in which could, without previous notice, make the statement untrue " (p. 46). That is to say, we must consider *all* the circumstances which are relevant to a given relationship, and find out *all* the circumstances under which the relationship in question does not hold. As a result we should be able to state the circumstances in which the specified relationship *will always* hold, and this statement obviously would be a verified law. If we ignore *any* relevant circumstance, we no longer have a valid law, but merely assumptions or hypotheses which necessarily contain " artificialities ", and are quite undependable for purposes of prediction.[1] This, as we noted, is Keynes' principle of limited variety (which, Keynes asserted, was implicit in Mill's doctrine).

Now, Schoeffler's thesis is that there can be no economic laws. In order to establish this, he tries to show that the economic system is " essentially open ", where an " essentially open system " is defined as one regarding which " . . . we have *conclusive evidence* supporting the belief that . . . " it can never be law-abiding (pp. 49–50, our italics). It is not surprising that Schoeffler holds that we can have " conclusive evidence " concerning the question of whether a system is law-abiding, because he appears to take for granted the principle of limited variety. Yet when, after a lengthy series of definitions, he attempts to show that the " system " is " essentially open " (i.e. that we have " conclusive evidence " that it is lawless), he admits that " this assertion is easier to make than to prove " (p. 51). This amounts (by definition) to the admission that the " economic system " is *not* " essentially open "; nevertheless, after failing to prove his point, he proceeds as though it had been proved.

Clearly we can never have " conclusive evidence " that a system is or is not law-abiding (just as we can never verify any law). Consequently, Schoeffler has not advanced a methodological criticism of existing economic theories, as he thinks he has done, but rather an alternative (metaphysical) economic theory, the consequences of which he himself appears to find implausible.

Professor Schoeffler's theory is that no economic laws exist. It would follow that all " economic variables " are independent of each other, and therefore, for example, that the price-level is totally independent of the quantity of money. He may claim that he can escape this consequence by asserting that in an " essentially open system " the " variables " need not be totally independent; a system is

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[1] Undergraduates who complain that the social sciences can never be " scientific " seem implicitly to share Schoeffler's notions of what constitutes a scientific law. " Artificialities " are false universal statements, like the hypothesis of profit maximisation. Since there are no economic laws, all economic models necessarily contain " artificialities ".

" essentially open " if the relationships among its constituent " variables " are known to be " too weak or too few " (p. 50). But this is clearly inconsistent. However weak or few the relationships are, on his own view we could state a law which describes just these weak relationships.

Professor Schoeffler's discussion of the failures of economics and his analysis of the reasons for these failures are completed within 54 pages. The next 100 pages constitute so-called case-studies, in which one economic model after another is proved to contain " artificialities ".[1] Usually the argument runs as follows: since the theory contains assumptions of economic laws of one kind or another, and since no economic laws exist, the model is false. This is, of course, *petitio principii* of the most obvious type.

We conclude that Schoeffler's attempted critique of economics fails entirely. Indeed, it seems to us utterly pointless to talk of " relevant variables " and " validity conditions " *except in the context of hypotheses*. Hence we can conceive of no other reasonable procedure in economics than the attempt to formulate hypotheses and to subject them to critical assessment. Schoeffler's alternative, namely the possibility of basing predictions solely on past information, is a myth which has been propagated by philosophers over the past two centuries. We find Hume's criticism of it wholly convincing. We are quite aware that some distinguished contemporary philosophers do not share our opinion; conspicuous among them Rudolf Carnap, from whom Schoeffler borrows his predictive model (cf. p. 185). This is not the place to debate this issue; moreover, Schoeffler himself grants that the application of his model is in any case impractical for the time being, if only for purely technical reasons. Still he offers the unabashed suggestion that economists should forthwith abandon their traditional (critical) method in favour of his.

VI

We have indicated our view of the proper place of methodological criticism at the beginning of this paper. We do not think this a particularly unusual view; indeed, it was stated by Robbins in his *Essay* when he remarked that " economics is not one of those social sciences which are always discussing method before proceeding to deliver the goods " (pp. 115–16). We have sought to show that the practice of methodological criticism in economics often goes counter to this view, and that the cumulative effects of this may be undesirable. In doing so we may have unwittingly given the impression that we regard all methodological criticism as futile and, by implication, that we have found little of value in the writings to which we refer. This most certainly is not our position, and in conclusion we may state our view of the proper place of methodological criticism.

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[1] It may be pointed out that some of the methods surveyed would hardly command the approval of most economists, e.g. some aspects of the statistical work of the N.B.E.R., some " business forecasting techniques ", and the Dow Theory of stock exchange price movements. There are quotations from E. R. Dewey and E. F. Dakin, *Cycles: the Science of Prediction* which would make most economists wince, and one wonders why Professor Schoeffler thought it necessary to include *this* example in his " case studies ". Even more surprising are his own indulgent comments on this particular work (p. 102).

First and foremost, methodological argument can be used to advocate the critical attitude, by trying to demonstrate its fruitfulness or by arguing against different approaches. Given that the critical attitude is adopted, methodology can further its practical application by the appraisal of the " status of particular propositions ". It is helpful to be told, for example, that one has unwittingly formulated a statement as a tautology, or rendered it untestable. It is useful to discuss explanatory power, informative content, and degrees of testability. (How much better Professor Papandreou's book would have been had he merely pointed out that the specification of parameters may increase the explanatory power and testability of some models). This sort of useful appraisal is, of course, to be found in the works we have mentioned in the earlier sections of this paper. Our objections were decidedly not directed against this kind of methodological criticism, but merely against the attempt to use this appraisal for the purpose of imposing rules intended either to enforce the critical attitude or, worse still, to undermine it. Above all, we contend, that it is important to guard against the illusion that there can exist in any science methodological rules the mere adoption of which will hasten its progress, although it is true that certain methodological dogmas, such as the dogma that only theories pertaining to measurement are significant, or the dogma of inductivism, may certainly retard the progress of science. All one can do is to argue critically about scientific problems.

The London School of Economics.

Book Reviews

Business Enterprise—Its Growth and Organisation. By RONALD S. EDWARDS and HARRY TOWNSEND. Macmillan & Co. Ltd. 1958. xvii + 607 pp. 60s.

Many of us are very ignorant on most industrial subjects. This is a simple consequence of two facts. Firstly, only a minority of those gainfully employed are concerned with manufacturing industry. Secondly, the number of changes of occupation in which members of the managerial classes indulge per career-time is a small one in comparison with the headings and sub-headings into which industry can be classified. Until comparatively recently this ignorance appears to have entailed few practical consequences. It is true that politicians customarily attacked or defended the management of industry and the behaviour of the wealthier classes. This amounted, however, to little more than political shadow-boxing. Threats to nationalise the means of production, distribution and exchange were at one time little more than verbal threats.

This situation has substantially altered since the war. Some industries have actually been nationalised. Proposals to nationalise others cannot be dismissed as vague threats. The question of what an industry is like before nationalisation and what it is transformed into thereafter is, accordingly, a question of practical importance. The technological backwardness or progressiveness of industry is likewise important. It cannot be disposed of at the level of political attack and defence. It involves no mere talking points but the balance of trade and the problem of Britain's survival among competitors determined that Britain shall not be the workshop of the world. In these changing circumstances it is essential to be able to see the industrial pattern as it really is, notwithstanding the fact that the reality is somewhat unlike either of the caricatures which it would be politically convenient to attack or defend. This being so, a number of economists have recently directed their attention to case studies, thereby placing at the disposal of the interested student evidence of what industry is really like. The contributions of Carter and Williams, equally with those of Jewkes, Sawers and Stillerman, have been reviewed here and elsewhere. The latest, and in a way the most interesting contribution, comes from Professor R. S. Edwards and Mr. H. Townsend, both of the London School of Economics.

In reviewing their work it seems appropriate to note first their qualifications for writing it. The main landmarks in Professor Edwards' career have been as follows: a training in and ten years' experience of accountancy; the intellectual adventure of learning economics and bringing its analytical tools to bear on accounting principles; six years in the Ministry of Aircraft Production; a leading part in the rebirth and development of the British watch industry; the writing of two books on industrial research and five years' membership of the D.S.I.R. Advisory Council, as it then was; membership of the Herbert Committee of Inquiry into the Electricity Supply Industry; and last but by no means least his seminar on Industrial Administration in which many of Britain's leading industrialists have during the last twelve years presented papers, taking as their theme the precise manner in which their firms originated, developed and are now administered.

The circulation of the seminar papers to previous speakers has been as popular with Professor Edwards' " Old Boys " as with his students, and their influence in giving executives from one industry insight into the problems of another must have been important. His co-author, Mr. H. Townsend, has also specialised in industrial economics having made a detailed analytical study of the cutlery trade in Sheffield before joining the staff of the London School of Economics.

Their book records, *inter alia*, the generalisations to which the Industrial Administration seminar has led. The result is a work of outstanding and meticulous scholarship dealing with the little explored no man's land which lies on the boundaries of economics, sociology and technology. The first chapter deals, as it should, with " The Birth of Firms " and here, buttressed with detail from the seminar papers, we get facts, not as they might be, or ought or ought not to be, depending on our prejudices or preconceptions, but as they are or were; as related in some cases by the founders themselves or, in others, by their descendants or successors. We can follow, for example, the history of Miss Winsor and Miss Molesworth who founded International Services Ltd. in 1945 with a capital of £300; or of Henry Simon who founded in 1879 the group of public companies that bear his name today. From such factual material there emerges a kaleidoscopic picture of the way in which the industrial mosaic has been laid; new firms entering existing industries; new firms with new ideas; new firms with imported ideas; new firms revolutionising existing industries; and new industries established by a free-for-all of old and new firms. The problem of initial capitalisation, particularly where it must be on the large scale or not at all, is appropriately treated together with the blend of chance and personality that make for early success or failure. There follow chapters on the growth of firms, the changing setting of business enterprise and the variety of industry. At this point the exposition could easily collapse into a mass of inchoate detail, but the authors have their material firmly under control and the blend of chance and causality begins to compose a pattern which can be comprehended.

What emerges is a mosaic but not a disorderly one. What has happened has happened because it was possible; the possibilities were bounded in scope but their intercombinations were unlimited in complexity. Chance and personality were final determinants within the limits of what was possible and viable. What emerge ultimately are the personalities of corporate bodies engaged in commerce and trade. Personality in fact runs through the book from first to last. The seminar papers were written by people about the activities of people. One feature was common to them all: they had somehow stood on their own feet in a competitive world where it was possible to fail, and had survived. The same was true of the firms directed by or employing them. They also had survived in a competitive world where failure was possible. Apart from this they had very little in common either commercially, administratively or historically.

In the second section of the work we are introduced to the factors which impose order on the mosaic and prevent it falling into chaos: cause and effect operating through the location of establishments; the effect of size with its concomitants, economies and diseconomies of scale; together with the integrating factors represented by administration, the market and co-operation. Here again corporate personality emerges as the resultant of time, chance and the individual personalities out of which it grew. The picture is not just of " big business " in the abstract, to be worshipped as the raw material of success, or abused as the egotistical exploiter of labour. One can say of Lever Brothers, I.C.I. or the Shell Oil Company that all three are big; but their bigness does not entail any sort of morphological homogeneity. They are different; their corporate personalities are different; their organisation is different. Which is cause and which is effect? As before, what has emerged has come to pass because it was possible; the possibilities were limited in scope by circumstances, but unlimited in the complexity of intercombinations; within these limits, time, chance and personality were the determinants. The result had to be appropriate in terms of these and the overriding need to survive.

In an appendix to this section we can compare the incisive mind of Mr. A. H. Wilson describing research and development in Courtaulds with the more administrative philosophy of Mr. O. W. Humphreys describing the same activities in General Electric. Why does neither record any counterpart to the statement of Mr. George Walker, Secretary of Associated Electrical Industries Limited, namely: " In 1946 we set up at Aldermaston a fundamental research establishment, in the charge of Dr. T. E. Allibone, F.R.S. This is one of the very few privately-owned laboratories of its kind in the world; its staff are not responsible to the factories for producing discoveries of commercial importance; they are concerned with the basic stuff of knowledge. They are several jumps ahead of the rest of the world in many obscure branches of—for instance—physics and metallurgy; and because we don't expect them to pay their way, they have paid it handsomely ". Compare this with a cognate expression of philosophy from Mr. Wilson: " The assessment of the economic importance of research programmes almost from their inception is one of the guiding principles to which I hold most strongly. The assessment may be vague and it is always difficult, but it should always be attempted since it brings one right up against the question, ' Why am I doing this particular piece of work ' ? " Mr. Humphreys likewise is quoted to the same effect: " . . . the chief scientist has always to have in the back of his mind that the ultimate purpose, however long-term, of the work under his care is to benefit the company which employs him ". No one could deny the commercial viability or technological zeal of companies such as G.E.C., A.E.I. and Courtaulds; nor could one deny the esteem in which Mr. Humphreys, Dr. Allibone and Mr. Wilson are held by those competent to appraise their standing as leaders of research. Mr. Wilson and Dr. Allibone are both Fellows of the Royal Society and have a common background of Cavendish Laboratory Physics. Respectable precedents can be quoted supporting each of the philosophies they represent; they are not idiosyncratic. Why then does one find himself at home with A.E.I. and another with Courtaulds unless the corporate bodies themselves possess to some extent corporate personalities? Personalities are not necessarily constant; they grow and evolve. It will therefore be interesting to see whether the Long Term Research Group recently started by Courtaulds comes in the end to resemble the A.E.I. Group at Aldermaston.

The third part of the book deals with the relations of government and industry, over half of it being concerned with the nationalised industries. Here again the authors are at pains to present reality as it is, and not as it might be, disguised under some tarnhelm of homogeneity. The nationalised industries differ from one another by at least as much as they differ from the private industries to which they stand in contrast. It may be that the problems of public industry are as interesting as those to be found in the private sector of the economy, but not even Edwards and Townsend can make them sound as if they were ! In so far as corporate personalities emerge they too often sound tired and disillusioned. Is this altogether a coincidence ? The complex mosaic of private industry represents spontaneous evolution in response to circumstances and people. Variety is the inevitable emergent if people and circumstances are left to themselves. This is not an individual conclusion. It seems to be the general verdict of social anthropology whether its studies be carried out on Polynesian Islanders or London Teddy Boys, and to most of us variety is the spice of life. Crush that variety organisationally, procedurally and technically, and you crush the life out of the corporate personalities associated with it. Can individual spirits survive the death of the corporate personality that they sustained ? It seems unlikely, yet in some contexts, coal mining for instance, there seemed no alternative to nationalisation. Given the existence of nationalised industries, Professor Edwards' concern (very properly in a Deputy Chairman of the Electricity Council !) is to ensure that they are operated on sound commercial lines. The author's defence of British private industry, however, is spirited and one suspects that it is his true-love even if he is too tactful to say so: " . . . it is time that someone put in a good word for the British industrialist. He faces many hard and perplexing problems and his life is not all beer and skittles. His social status is still uncertain. Society's view of the industrialist and of his job, and the industrialist's view of himself, are of very great importance to recruitment and to morale. In the main, people tend to live up to what is expected of them."

In passing to the conclusions the informed student will read on with steadily increasing pleasure. A balanced, fair-minded appraisal of Britain's industrial future emerges expressed in a lucid and readable prose. " . . . the organisation of society should be such that individuals face the real cost of the choices they make; . . . every individual should be taught as far as possible to recognise and understand these choices for himself. . . . Competitive industry ensures that any person shall be free to offer a service that others might wish to buy. The essence of free enterprise is that he shall not be stopped by someone else who does not wish to supply the service. . . . Three points sum up much of what we have learned from writing this book. First, the patterns of industrial organisation are bewilderingly complex, and it would be a matter for concern if they were not. Secondly, no assumptions should be made or action taken that would imply a bias in favour of either large or small scale. Thirdly, the State should, in general, avoid action and prevent action that would bring about rigidities in the industrial structure. The most important requirement of industrial organisation is that it should provide for variety and change."

" Business Enterprise " is not an encyclopedia. It is not a reference book. The pathology of the subject is entirely omitted, intentionally so, for pathological individuals must be clinically anonymous. But as an introduction to the normal anatomy of commerce the book covers an exceptionally wide field. What it expresses is a philosophy and an outlook based on factual illustrations culled from the world of commerce and technology as it really is, perceived as a whole and comprehended and presented as such. The nation has suffered overmuch from criticisms of industry based on inadequate appreciation of what industry is really like. Here at long last is a work of scholarship which can supply the necessary corrective.

HALSBURY.

Problems of Capital Formation. The National Bureau of Economic Research. Princeton University Press. Oxford University Press. 1957. xi + 612 pp. 60s.

This volume contains the fifteen papers delivered at a meeting in 1953 of the Conference on Research in Income and Wealth, together with a number of comments by participants. It is impossible to enumerate, much less do justice to, the various contributions, which cover a very wide field and are all of considerable interest.

Four papers deal with capital coefficients and either seek to measure them in various American industries or to account for variations within the same industry at a point in time. These papers are of particular value to the non-specialist in helping him to grasp the inter-connection of input-output concepts and the concepts of traditional capital theory. They shed little light on secular trends in capital coefficients, a subject that is also neglected by the other contributors; but they form a useful background to the subsequent discussion of the acceleration principle in papers which discuss this principle either directly, in relation to the creation and utilization of productive capacity, or more obliquely, in relation to fluctuations in stocks.

The efforts of Modigliani to rehabilitate the acceleration principle are of special interest and involve the introduction of three coefficients about which we know too little and which can hardly be treated as constants: the time lag between decisions to increase capacity and the completion of the additional capacity; the expected rate of increase of output; and the urgency of the measures taken to make up deficiencies in capacity. With the help of these coefficients, Modigliani is able to arrive at a plausible model and submit it to a rough test. But the more one reads Carter on the variability of capital coefficients and Eisner on what business men say they do, the more doubtful one becomes of the predictive value of the principle in conditions of discontinuous technical change.

In the papers on stocks, of which there are three, it is again Modigliani who is the most penetrating. He analyzes the reasons why stocks are held and demonstrates that, whatever the avowed purpose, the main forces giving rise to the holding of stocks are such as to make the optimum stock a function of the level of sales. A somewhat similar line of thought is developed by Robert Eisner in order to demonstrate that when business men belittle the influence of interest rates on their investment plans, they may give a correct account of the specific influences dominating their individual decisions and yet convey a false picture of the really important influences on *aggregate* industrial investment.

Of the problems pursued in the other papers, the most interesting is the treatment of capital consumption and depreciation. This crops up in a paper on residential construction where, to use the jargon of modern physics, the half-life of a house (the age at which it has depreciated by 50 per cent.) is put at about thirty-five years. It crops up again in George O. May's outline of changes in the accounting treatment of capital items over the last fifty years and in an attempt by Grosse and Berman to find a method of estimating replacement purchases of capital equipment. It also forms the subject of an outstanding paper by Edward F. Dennison on the implications of changes in the design and quality of capital goods for the measurement of capital formation and capital consumption. Although the outcome of the paper is an endorsement of the perpetual inventory approach, it brings out some of the less satisfactory implications of this approach.

Dennison starts from the familar issue of what is meant by keeping capital intact and concentrates on two alternative measures of capital, one looking backwards to cost and the input of physical resources, and the other looking forwards to the value of capital goods and the flow of productive services which they are expected to yield. Everybody agrees that, in the absence of technical change, the replacement of one machine by an identical machine keeps the stock of capital intact. But what if the cost of making machines falls or the productive efficiency of machines rises ? In the first case, the same rate of saving—if the cost of consumer goods is unchanged—makes it possible to build more machines. Dennison treats this as an increase in capital formation; although, since the input of real resources is unchanged, not everyone would agree. In the second case, better machines are substituted at no increase in cost. Dennison argues that this may or may not rank as an increase in capital formation according as one uses a forward-looking or backward-looking measure. He ends by ruling out the forward-looking measure because in the real world it cannot be used; it is impossible to segregate the contribution to future productivity made by an increment in capital from all the other influences at work. He is therefore forced back on a measure using reproduction cost at some base date and ignoring year-to-year changes in quality.

Improvements in design, quality, length of life and so on are thus to be left aside while reductions in the cost of capital goods are taken fully into account—hardly a very satisfying solution, since the economic effect of an increase in productivity is identical with that of a reduction in cost. Moreover it is impossible to leave out obsolescence and this is, as Dennison recognises, simply one form of quality change. Dennison meets this difficulty by suggesting that obsolescence should be deducted from gross capital formation on the retiral of equipment not fully depreciated. He does not favour the use of capital adjustments (e.g. when obsolescence is particularly rapid) as this would disturb the principle that over the life-cycle of a piece of equipment capital formation and capital consumption should balance. But not only must his solution yield misleading results (particularly if current depreciation is based purely on wear and tear and makes no allowance for gradual obsolescence); in the end the capital stock of the community must be subject to windfall changes that can only be reconciled with past costs through capital adjustments. There is likely to be a cumulative divergence between estimates of capital based on variants of the perpetual inventory approach and estimates based intermittently on market valuations of future yield, and this divergence will be the greater, the more rapid the rate of technical progress. Economists do not measure capital only for the purpose of settling problems of attribution between factors or of establishing the rate of growth of productive capacity; they cannot treat capital simply as an aggregate of past inputs if, for example, they are comparing the rate of growth of capital and income, and this is all the more true if quality changes are disregarded in the measurement of inputs.

The relation between obsolescence and depreciation also needs rethinking if depreciation frequently takes the form of a gradual relegation of equipment to less important uses either through sale or within the same firm. As is pointed out by Domar, " a large, and perhaps a major part, of industrial capital is so well maintained that its productive capacity hardly declines with time; it is retired in response to technical obsolescence rather than to actual deterioration ". But if so, why is so much of the theory of capital consumption conceived of in terms of wear and tear over a fixed life and so little in terms of stand-by plant ? The more it is obsolescence that produces retiral, the more the longevity of capital can vary and the more important it becomes to have some knowledge of fluctuations in longevity and their causes. Current estimates of capital formation often assume a constant life for capital goods while, in Britain at least, many types of capital goods have been retained in use for longer periods than before the war. It is therefore surprising that estimates of depreciation based on an unchanged length of life have been allowed to go unchallenged. Can it really be true, for example, that depreciation in Britain is nearly one-third higher *in relation to gross national product* than before the war ?

Dennison insists throughout that it is capital consumption rather than the capital stock that matters. Perhaps he overstates his point; but it is certainly time that we had a full-scale treatment of capital consumption that brought together the known facts and gave the theory a more practical turn.

A. K. CAIRNCROSS.

The Economics of Sir James Steuart. By S. R. SEN. G. Bell & Sons for the London School of Economics. 1957. vii + 207 pp. 25s.

Steuart's writings have always, until now, been a remote and inhospitable corner of economic literature. The main work, his *Principles of Political Oeconomy*, is as drily unpleasant a piece of writing as one can easily find among the output of the eighteenth century. And leaving aside all æsthetic qualities, a sacrifice to which economists have by now inured themselves, Steuart's doctrines, as such, have made his work seem arid. Although he wrote just before Adam Smith, the *Principles* being published only nine years before *The Wealth of Nations*, there appear in his work few hints of the classical analysis, so that he cannot be mentioned in the same breath with such great predecessors as North, Cantillon or Hume. On the other hand, he comes much too late in the day and is too irregular to be classed with the " standard " mercantilists, among whom his great penchant for governmental control of economic life would seem to place him. He has enjoyed just such deference as one would pay a superannuated colleague, of obscure but certain merit, whose turgid, confusing and outdated conversation one studiously avoids.

Mr. Sen has now undertaken to revise this attitude toward Steuart. On the sore point of literary style, indeed, he concurs in the prevailing view that the *Principles* is " an extremely difficult and tiresome book to read ", and is obliged from time to time to concede that Steuart can be " pedantic and hence obscure ". To overcome this obstacle, he has presented us with a skilful and clear restatement of Steuart's thought, which will be an invaluable guide to future readers.

But beyond this, Sen proposes a new theory of Steuart's position in the history of economic thought. He will not allow that Steuart was a somewhat deviant mercantilist, who had the misfortune to be advocating that policy just the moment before its intellectual respectability was destroyed by Adam Smith, but maintains, on the contrary, that despite any similarities between his and mercantilist thought, Steuart was something quite different. That he was not even partially an advocate of laissez-faire is too obvious to contest. The position at which Sen locates Steuart, therefore, is near the source of that line which began at mercantilism and " was later to lead to economic socialism " rather than individualism. The sort of socialism which Sen has in mind becomes clearer in his conclusion, that the present economic policy of Britain " is in effect neither Adam Smith's nor Karl Marx's but essentially James Steuart's ", and in his choice, as a name for Steuart's theory of economic policy, of the title, " Economics of Control ".

This is certainly a refreshing hypothesis for our time. Several broad histories published during this century have clarified for us fairly well what Mercantilism and Liberalism meant as ideal systems of thought and policy. Detailed studies of particular writers have shown how nearly impossible it is to find either of these doctrines consistently represented in the work of any writer between the beginning of the seventeenth and nearly the end of the eighteenth centuries. In view of all this it is a hopeful sign that Sen refuses to identify Steuart with either camp. But if Steuart was neither a mercantilist nor a liberal, it does not follow that he was a socialist, or planner, or intellectual predecessor of Keynes and the welfare state. The comparison of contemporary socialist policies with the policies advocated by those whom we unflinchingly call mercantilists shows similarities as close as those that Sen points out. Petty, for instance, was advocating nothing new when he proposed laws and policies of which the Direction of Labor Act can be considered a distant echo, nor was he original in suggesting that taxes could be used as a method of regulating consumption and investment ; neither were nationalized industries unknown nor unadvocated by mercantilists. It would be an error, however, to deduce from these resemblances that modern socialist policy is a lineal descendant of Petty or of mercantilism. It may be equally erroneous to hold that because Steuart's proposals were not like some other mercantilist proposals and not unlike certain socialist policies, he ought to be regarded as a sort of socialist rather than as a mercantilist. In which camp he should be placed depends finally on the use to which one intends to put the classification. For some purposes it might be most appropriate to treat him as a socialist, for others as a mercantilist, for still others as a Scot, advocate, Jacobite, exile, or advisor to the East India Company. Such questions of classification can never be closed once and for all.

Yet whatever view one takes of Sen's broader judgment on Steuart, one cannot but be grateful for his painstaking and successful effort to make Steuart more palatable, thereby attaching some interest and pleasure to the reluctant duty we have always felt to read him.

W. L. LETWIN.

Economic Analysis and Policy in Underdeveloped Countries. By P. T. BAUER. Duke University Press, Durham, N.C. Cambridge University Press. 1957. xiii + 145 pp. 22s. 6d.

While even a voracious reader of literature on less-developed countries might quail at the thought of yet another book on the subject, he is bound to make an exception in the case of Dr. Bauer who can be expected to say the unexpected. This little book, which represents three lectures given to the Commonwealth Studies Centre at Duke University, goes over much the same ground as his joint text with Mr. Yamey, but nevertheless offers its own distinctive contribution to the methodology of economics applied to backward economies.

Dr. Bauer begins by offering convincing evidence of the applicability of general economic analysis to the problems of backward economies, contrary to the view of many theorists and administrators. As he shows, the inexorable law of supply and demand works even under conditions where institutional rigidities appear to be paramount. The human propensity to truck and barter soon develops into a strong urge by native producers to operate in cash sectors, even in the teeth of official disapproval backed by the law. Mr. Bauer's striking examples can be extended indefinitely, the reviewer's favourite being the case of illegal sales of coffee in Tanganyika which amounted to as much as one quarter of total output in 1953. Paradoxically, those who most stress the differences between Western-type and less-developed economies are often those who ignore the wide diversity of economic activities in the latter because of their relative unfamiliarity. No one would deny the relative poverty of many Colonial territories, but it is very easy to underestimate their income per head, largely by ignoring the so-called subsistence sector in which even capital improvements may be undertaken by peasant farmers. As Dr. Bauer shows, there are many other features of the economic landscape which are ignored or ill-perceived even by observers on the spot.

In his final lecture on " Issues of Policy ", Dr. Bauer relates the depressing story of the inconsistencies of official policy arising largely out of lack of expert appraisal of the likely effects of piecemeal intervention. He ends with a passionate attack against forcing less-developed economies to institute compulsory savings measures against their will. Even allowing for the necessary process of selection which produces a stimulating lecture, Dr. Bauer would perhaps strengthen his case, or a better understanding of the economic issues, if he could also produce examples of enlightened as well as ill-considered public policies. One wonders also whether it is in fact generally true that forced savings through taxation for development purposes is solely the reflection of the " insensitiveness of humanitarians "; such measures may have popular support even if the implications of policy are not fully realised. These and other reservations (Dr. Bauer is a little hard on economic soothsayers) are not meant to cast doubt on the quality of the exposition, and the reviewer is bound to agree with the foreword of Professor Spengler that these lectures are " penetrating and original ".

ALAN T. PEACOCK.

International Trade. By WENDELL C. GORDON. Alfred A. Knopf, New York. 1958. xviii + 647 + xix pp. Charts. Tables. $6.75.

In his preface Professor Gordon of the University of Texas says his book has three aims—to integrate current thinking in the field of international economic relations, to suggest a co-ordinated pattern of policies which would make possible the existence of a viable world economy, and to be readable by laymen and students. While it is rather doubtful whether those without two years of economics behind them would appreciate much of the book, those with that minimum should find Professor Gordon's treatment stimulating. Each chapter presents its material in a way well suited to class or group discussion. Although the book was intended for an American public (see reference on p. 567) some of the material, such as that concerning pressure groups behind the U.S. tariff (chapter 10), will probably appeal as much elsewhere.

There is a broad liberal outlook running through the book from the early discussion of modern theory and the brief statement of the case for what is called " constructive free trade " to the later plea for an international currency. In following through the requirements of a viable world economy Professor Gordon shows particular interest in problems of economic development, often citing a Central or South American example. Thus he asks, apropos of Mexican experience, whether inflationary financing is as necessary for economic development as it is currently alleged to be. He goes on to consider the basis for extending credit in areas of uneven development and potentialities. Here Professor Gordon does not appear to be of one mind. For on p. 418 he states : " And the criterion in deciding how much credit there is to be should not be financial but technological. What development is technically feasible and desirable ? Financing should be made available for all such projects ". On p. 553 he declares : " Nor is it enough to say that engineers, taking technical considerations alone into account, could satisfactorily guide production if the institutional barriers could be eliminated. What criteria are *they* going to use in deciding whether a certain development should or should not occur ? " He is anxious to remind us that, despite what some " experts " who are accommodating to underdeveloped economies allege, it is in fact easy to exaggerate the net contribution that foreign investment made in the past to developing economies. He adds : " the self-financing of capital goods imports by means of an export balance of trade might make for more of a real net contribution " (p. 451). Later on he concludes that " interest rate and profit rate differences may or may not exist as a superficial explanation as to why foreign investments occur. The more important conclusion as to the cause underlying foreign investments is that their growth is largely the result of the relative development stages of the various countries " (pp. 531-2). The argument is reminiscent of that of Keynes and Macmillan which alleged that Britain was institutionally biased in favour of foreign investment in the period to 1914. Be that as it may, there seems to be a good deal less evidence for this since 1914. Tax concessions and tariffs now create a " pull " in place of any earlier institutional " push ", except where extractive industries are involved.

This leads to a compressed 90-page section on Growth where the author brings the various theories of development under brief but usefully critical review, makes some arguable generalizations (*e.g.* " any country that has a real monoculture and diversification problem is *too small* " p. 560), and refers to the empirically suggested (and suggestive) proposition that over recent years output has increased mainly because of an increase in output per unit of capital rather than a great increase in capital.

The book concludes with an 8-page chapter on the co-ordination of policies. In the space the author can do no more than set up a few signposts. One is left to wonder whether the author feels that progress could and should be made in Europe and elsewhere along the road of regionalism.

D. J. MORGAN.

Common Sense about the Common Market. By E. STRAUSS. George Allen & Unwin Ltd. 1958. 168 pp. 15s.

Up to now most of the literature dealing with the plans for free trade in Europe has concentrated upon the economic issues raised by such a departure from traditional policy. In this book, however, Mr. Strauss goes one step further and discusses the repercussions of closer economic integration upon the balance of power in Western Europe, and, in particular, upon the relative positions of France and Western Germany. His main thesis is that the creation of the Common Market will add a further stimulus to German economic, and hence ultimately political, supremacy on the Continent. He therefore argues that British participation in European free trade is essential for the long-run stability of Western Europe, though he does not regard the Free Trade Area as a promising instrument for associating the United Kingdon with the " Six ". Rather, he is looking for a wider preferential trading area, embracing the Common Market, the remaining O.E.E.C. countries and the Commonwealth. In his scheme only the strongest economies would forgo tariffs and quotas altogether, whereas the weaker members would be permitted to retain a certain measure of protection, so as to ensure that all countries–not only highly industrialised Britain and Germany—experience continued development and growth.

This line of argument, presented in the last few chapters of the book, is preceeded by a review of the role of " common markets " (especially the Zollverein) in German history, by an account of postwar German expansion, and by a critical survey of the European Coal and Steel Community, the Spaak Report, and the Rome Treaty. The upshot of this early discussion is, according to the author, that this is not the first time that Germany has tried to extend her economic and political supremacy by means of economic unions with her prospective victims, and that the supra-national Common Market institutions will be too weak to cope with this latest surge for power, just as the High Authority of the Coal and Steel Community failed to impose its will upon Germany's basic industries. This is not to say that in the author's view a dark and secret plot is being hatched in Bonn for achieving by means of economic integration what Hitler failed to bring about in six years of war; rather, Germany's big business interests, in their search for ever bigger and wider markets, are regarded as the villain of the piece.

It is a pity that, as the book reveals very clearly, Mr. Strauss sets out on his venture with a number of preconceived ideas firmly fixed in his mind. Perhaps the most important of these is a deep distrust of the free play of market forces in a modern capitalist society—hence his distaste for the Free Trade Area, with its reliance upon the working of competition. Also, his reasoning (especially on economic issues) is not always convincing. The French economy in particular does not appear, from a long-run viewpoint, to be anything like as weak as Mr. Strauss would make it out to be. In addition, the author appears to lack a full appreciation of more theoretical points, such as the role of the exchange rate mechanism, or the effects of free trade upon employment.

On the other hand, many of the more general arguments appear to be well taken. His insistence upon the importance of giving fuller recognition to the position of Europe's underdeveloped areas would appear to be fully justified, and, whilst one cannot help doubting the realism of his proposals for the extension of imperial preference, he is surely on the right track in emphasising the limitations of any integration on a purely European basis, and in pointing instead to the great advantages of associating the Commonwealth countries with the movement for free trade, even though this can at best be only a very long-term proposition.

H. H. Liesner.

Japan's Economic Recovery. By G. C. Allen. Oxford University Press for The Royal Institute of International Affairs. 1958. 215 pp. 25s.

Professor Allen is both authoritative and gifted as an expositor of the course and problems of economic progress in East and West. This is another work of sound knowledge and penetrating analysis, giving a useful and vivid account of the postwar recovery of Japan's economy, in fact of the nature and problems of the Japanese economy more generally. There is a useful statistical appendix, and the whole is well indexed.

After a period of inflation and confusion (1945–49) under an Occupation Authority concerned (somewhat brashly) to suppress all totalitarian tendencies in economic as well as political life, and to impose reforms which would engender democratic tendencies in their stead, financial stability and a new rationalisation of the economic structure emerged in 1949–50. Then came the Korean War boom—a tidal wave of foreign aid, investment and inflation, bearing the Japanese economy up to plateaux generally higher than the pre-war levels—lasting till late 1953. In 1954 inflation was checked and exports at last began to revive, but on a pattern distinctly different from the pre-war one. From 1955 onwards there was rapid growth in all sectors of the economy including the export trade.

Against this phase-sequence Professor Allen very ably fills out the rest of the framework: the population problem is less formidable than before, its stabilisation perhaps in sight, labour costs are high, but the economy appears more diversified, and even more flexible or adapable than before. Detailed outlines follow on the monetary and banking systems, the agrarian sector, industry in general and textiles, engineering, black metals and power in particular, the *Zaibatsu* (big business) and labour, and the problem of foreign trade.

The survey being very condensed, though judicious, detailed criticism is impossible in a brief review. Little in the functioning of the Japanese economy escapes the eagle eye of Professor Allen; and he is an Olympian who keeps telepathically close to the mortals concerned. But the reactions of the Japanese themselves to the situations and factors he so clearly exposes are generally much more subjective and much less highly consequential than might be inferred from such an account as this. A *post hoc* summation, drawing largely on information of the "blue book" and "white paper" types, it oversimplifies many of the motivations and the actual responses. The Japanese behaviour pattern has not been by any means so pragmatic and rationalistic as this account tends to suggest.

Surprisingly, Professor Allen's last two pages are like a little coda on the theme of the present "perplexities" of the Japanese and the uncertainty of their future behaviour, in strong contrast to what they themselves would call the "logicalism" or objectivity of the whole preceding account. In the different metaphor of Nakayama Ichiro, the "Face of the Japanese Economy" (*Nihon Keizai no Kao*, 1953) is not shown at all, and its physiology not too well, by X-ray pictures. Professor Allen has given a masterly account of the mechanism, but hardly introduces its Japanese operators, except in this postcript. He does not wish to go far into questions "which a foreigner can hardly consider without presumption" (p. 189). His book fills an important gap—and shows up another, which it is desirable that the Japanese themselves should fill, by making their own thought and work better known abroad.

E. S. KIRBY.

Survey Methods in Social Investigation. By C. A. MOSER. William Heinemann Ltd. 1958. xii + 352 pp. £1 15s. 0d.

Field surveys dealing with social and economic problems have come to be used increasingly by social scientists and market researchers, and the design and execution of such surveys has become almost a new specialism of its own. The practitioner of this craft must be a versatile person and feel at home with such diverse subjects as sampling theory, questionnaire design, data processing and interviewing methods, including the recruitment, training and control of interviewers. This book is an introduction to these subjects, addressed partly to the student of the social sciences who may need the information for the purposes of an *ad hoc* survey, and partly as a guide to intending practitioners.

Different aspects of the subject have, of course, been developed to very different degrees. Mr. Moser begins with a chapter on the nature of social surveys, gives us an outline of their history with special reference to developments in this country, and then proceeds to a discussion of sampling methods. It is in this section that he is most successful. His account is clear, concise and accurate, yet sufficiently elementary and devoid of unnecessary technicality to be comprehensible even by those who have had no formal statistical training. It will prove most useful to students.

The fact that the remaining parts of the book seem less satisfying is due to the nature of the subject, rather than to any deficiencies on the part of the author. As Mr. Moser suggests in his conclusion, sampling techniques have almost ceased to be a problem in survey work. But the absence of a coherent theory becomes painfully evident when he deals with questionnaire design, interviewer training and response errors. There is a good deal of scattered experience and a voluminous literature through which Mr. Moser conducts the reader with great skill and discernment. But, at the end, one is left with a doubt as to whether the journey has been really worth-while. The general principles which emerge are often tautologous, even though their authors sometimes attempt to hide their true nature behind a grand-sounding terminology.

Perhaps the author might have emphasized the distinction between surveys designed to elicit purely factual information and those which aim to describe attitudes and opinions rather more strongly. A critical examination of the fundamental assumptions on which attitude surveys are based would also have been welcome. The careful reader will find himself warned against many types of error ; he is shown that the wording of a question and its position in the questionnaire might affect the result, that response errors, coding errors and the personality of the interviewer might be important, and that there may be considerable variation among interviewers. Yet the basic validity of the technique is not questioned. Mr. Moser admits that there are deficiencies and that considerable improvement is needed, but the limitations of the technique itself are not discussed.

However, it would be unfair to end on a note of criticism, for Mr. Moser set out to write a book on techniques, not on methodology. As a guide to survey practice and the sample theory which underlies it, the book may be recommended without any reservation whatever.

E. Grebenik.

Economic Arithmetic. By Robin Marris. Macmillan & Co., Ltd. 1958. xvii+349 pp. 28s.

This is a new type of textbook on applied economics. It starts with a description of the economy of the United Kingdom as it is revealed in official and other statistics. It then introduces the reader to simple statistical techniques for dealing with time series and finally discusses measurements of change. It is different on the economic side by not discussing demand, supply and elasticities. It is different on the statistical side in not developing ideas of central tendency and dispersion or tests of significance, unless indeed index numbers are looked upon as being simply weighted means. The book therefore is not an attempt to teach people to look for cause and hence is far from being a complete text on economics. It does, however, teach people to look at the real world and to know how to study the relationships and changes therein. Such integration between economic description and statistical interpretation might well preceed attempts to find reasons and be a good approach to inductive economics.

The first quarter of the book, called " Anatomy ", is primarily a description of national income statistics and, though it is illustrated by two " flow " diagrams, the approach is static. The diagrams show flow, but are balanced and give no picture of change. Such an introduction is probably necessary but it gives an impression of being distinct from the major part of the book which is concerned with measurements of change.

The instruments used for dissecting the " anatomy " are regression analysis and index numbers. The dissection therefore is a dissection of time series rather than of the body to which we have been introduced. However, it is well done. There is a delightful example, and a horrid warning, of how the archaeologist of the year 3000 might come to very convincing but entirely erroneous conclusions about the twentieth century domestic tariff for electricity by fitting a straight line to data of units consumed and amounts charged in houses of different sizes.

The last part of the book deals with measurements of real change. We have, of course, had index numbers with us for a long time. They used to measure temporary variations, primarily caused by the trade cycle, from normality, and normality was stable prices. Prices of 1850 would have been recognisable in 1914. The price rise associated with the 1914-18 war was an aberration; between 1924 and 1938 it was again possible to look upon stable prices as normality. It is only some twelve years after the end of the 1939–45 war that we begin to wonder whether normality does not mean fluctuations about a continually rising price trend. It has now become absurd to make comparisons over time in money terms and we have had to think seriously of scientific measurements in real terms.

The last part of the book discusses measurements of change in family real income and real national product. The problems are not all problems of constructing suitable index numbers; there are many conceptual problems as well. Some of these arise from the use of resources to produce capital equipment which will yield consumption goods only in the future. To discuss this Mr. Marris introduces a " capacity " concept which will require to be used with care as it seems at variance with normal ideas of working to capacity. Many problems arise from the activities of the Government in providing goods and services paid for out of compulsory taxation. There is a very worth-while discussion of the real value of government expenditure on defence, administration, education and health. It is perhaps at present more important to realise the problems than to quarrel with the way they are usually treated.

H. S. BOOKER.

The Trades Union Congress, 1868–1921. By B. C. Roberts. George Allen & Unwin Ltd. 1958. 408 pp. 35s.

Various studies of British trade-union history have been published, but if we except the reminiscences of an old unionist which appeared many years ago, Mr. Roberts is the first author to devote himself to a special history of the Trades Union Congress itself. The subject, naturally, is of considerable importance: and Mr. Roberts accomplishes his task successfully. He has not, it seems, been able to use the minutes or files of the Parliamentary Committee, the predecessor of the present-day General Council; but experience usually shows that working-class organisations keep very limited records of their more intimate proceedings, and the bulk of the evidence is usually to be found in annual conference reports or in newspaper accounts. These sources Mr. Roberts has fully utilised.

It is interesting to observe today that the T.U.C. was founded largely as an agency for propaganda among the educated classes. Its early meetings were modelled on those of the Social Science Association, and consisted of the reading of papers designed to justify the existence of the unions to genteel public opinion. Even as late as 1900 the Congress was subjected to " an interminably long and boring lecture on the anthropological theories of Darwin, Huxley and Spencer ", which could apparently in some way be marshalled to the defence of trade unionism. The Parliamentary Committee that became the Congress's instrument for lobbying M.P.'s and appealing to public opinion naturally had to concern itself at first with a Parliament in which working men had no direct representation. Consisting as it did of the older and more established union secretaries, the Committee's attitude was usually one of cautious deference. Only gradually, with the extension of the vote, the growth of the " new unionism " and of working-class Socialism, and the formation of the Labour Party, did the Parliamentary Committee begin to claim a permanent foothold for itself in both Whitehall and Westminster. The First World War enabled it to acquire and to consolidate a " respectable " status as the representative agency of the industrial workers, the majority of whom were now organised in its component unions.

The year in which Mr. Roberts concludes his volume, 1921, was the year in which under a scheme of re-organisation the Parliamentary Committee was abolished and the General Council substituted in its place. We can see in this change a recognition of the importance of a nationally co-ordinated industrial strategy—a recognition that owed much to the work of the Syndicalists and the Guild Socialists, and that was to lead to the climax of the General Strike in 1926. One of the most interesting stories of the evolution of the T.U.C. is thus interrupted some time before the denouement, and we must wait for Mr. Roberts' second volume to have the benefit of his version of the events.

There is much to commend in Mr. Roberts' treatment of his subject. He is discriminating in the use of sources. He makes a better case for George Potter, the editor of the *Beehive*, than the Webbs ever allowed; and he rightly discounts the historical value of Mrs. Webb's entertaining but often erroneous diary. He tells us something, though perhaps not enough, of the way in which the larger unions engaged in " horse-trading " with one another in order to secure the election of their men to the Parliamentary Committee; and of the bitter resentment that this created among the ambitious leaders of the smaller unions. It was a process which a writer in the Socialist weekly, the *Clarion*, rather uncharitably compared to a parliamentary election in the rotten borough of Woodstock. Occasionally perhaps an opportunity for a telling and vivid sketch of the changing social pattern is missed: for instance, both John Burns and Will Thorne wrote accounts of the quarrels between the " new " and the " old " unionists at the Liverpool Congress of 1890, at which the " new " unionists were making their first appearance; these accounts, with their descriptions of the contrasting garb of the delegates, would certainly bear quotation.

In general, however, we must be grateful for this authoritative account of the early history of the T.U.C., with its useful appendices summing up its development; and we look forward to its sequel.

HENRY PELLING.

Australian Company Finance—Sources and Uses of Funds of Public Companies, 1946–1955. By A. R. HALL. Australian National University, Canberra. 1956. x + 198 pp. 32s. 6d. Aust.

This is indeed a " massive pioneering study ", as Mr. R. F. Henderson describes it in an introductory preface. Years of work have gone into the numerous tables based on a study of Australian public company accounts and much has been done to fill in one of the major gaps in Australian economic statistics, viz. those relating to the " corporate sector " of the economy, with particular reference to the sources of finance for industrial investment—depreciation allowances, retained profits, bank advances, new issues, and borrowing from parent companies overseas. Such a survey also raises important implications for fiscal and monetary policy, since some sources of funds are more amenable to regulation than others, and where (as here) figures are presented for a period of years the character of the changes taking place and the direction of economic growth can also be established. Obviously, the value of statistical analysis depends on adequate basic material and, as Dr. Hall is well aware, there are many pitfalls in company accounts. Yet it is only by subjecting such sources to careful analysis that the deficiencies can be highlighted and the long uphill educational battle for more adequate data commenced. The main emphasis in the present work has deliberately been placed on sources rather than uses of funds, since the author is more interested in the nature of the Australian capital market than in the processes of investment as such, though the " demand for funds and the type of funds supplied are . . . intimately related to the uses to which they are being put ". There are, too, some interesting possibilities of substitution.

Since he has chosen to work in what is very largely a virgin field, Dr. Hall has had to resolve as best he might some tricky problems of method and definition, but never has he permitted himself the luxury of rash generalisation and he is most cautious in interpreting the processed material. Nevertheless, he believes that it is possible to reach some " fairly reliable conclusions about changes in the patterns of finance " and these are set out chronologically, together with some international comparisons. For the general economist these are the most interesting chapters, though because of their compression they scarcely make light reading and greater fluency would have added greatly to the interest—and possibly the influence—of this book. As it stands, it is very much a work for the specialist and far too narrow a view has been taken of the effects of inflation, the impact of the recession in 1951–2, and the revival and recovery of 1953-4. In addition, Dr. Hall might have been tempted to delve into the theoretical implications of his findings, since it is only on the basis of factual enquiries like this that the validity of theory can be tested. Perhaps he can be persuaded to attempt this task at some future date.

J. S. G. WILSON.

Theoretical Economic Systems—A Comparative Analysis. By WALTER S. BUCKINGHAM, JR. The Ronald Press Company, New York. 1958. viii + 518 pp. $7.

This is not a textbook of formal economic theory. Rather is it a survey of those aspects of economic thought which treat of the working and development of contemporary economic institutions and of alternative economic systems. The arrangement is not chronological, however, but is by schools of thought which, though it entails some sacrifice of perspective, is handy for compact reference. Part I is a methodological introduction. Part II, on " Theories of Capitalism ", covers Classical and Neo-Classical theories, German historical theories, American Institutionalist theory, Socialist theories of Capitalism, Keynesian theory, and monopoly theories. Part III deals with various theories of socialism, and Part IV winds up with the writer's reflections on the evolution of existing institutions and the integration of economic theories concerning them. It is of necessity synoptic in style and, at the beginning at least, unduly simplified. The 33-page section on English Classical and Neo-Classical theory suffers most from this compression and may be responsible for several rather sweeping statements, for instance, that on page 55 which informs us that by the mid-nineteenth century the Classical economist " continued to praise individualism and to oppose interference with even the most outrageous exploitation ", a statement which would not be difficult to dispute. The exposition throughout is too popular to reveal the scope and growing rigour of modern theoretical analysis or to do justice to the acumen of the many writers whose views are summarized and appraised in this book. None the less the factual discussion of modern big business and its various interpretations by different political economists, Keynes, Schumpeter, Galbraith, McCord Wright and others, should serve well to arouse the interest of the beginner. While affording him occasional relief from pursuing the seemingly elusive models of his textbooks, it may profitably add some useful background to his knowledge of the subject.

E. J. MISHAN.

The Economics of Communist Eastern Europe. By NICOLAS SPULBER. John Wiley & Sons Inc., New York. Chapman & Hall Ltd. 1957. xxviii + 525 pp. £5.

Professor Spulber has sought in this book to assess the post-war economic development of Eastern Europe in terms of the uniform planning structure imposed on it. He has brought together a great deal of material on economic policy in a group of countries of differing income levels and circumstance. One of the first things which emerges, however, is the similarity of the general scheme in all countries. This derives from the restrictions of the Marxian scheme and the demands of the Soviet economic and political system. And the author concludes : " The uneven development of the region, the very unequal capacity for investment of the countries of the area, their reliance on those capacities, and the planning of each country for all-round development even during the second planned quinquennium will tend to engender substantial conflicts between these countries and the Soviet Union and among themselves ".

A central part of the ideological strait-jacket was the stress laid on heavy industry. This is expressed in the growing ratio of capital goods to consumer goods output. In general the process seems to have been carried on not merely outside the wider international division of labour, but also without such division within the region, other than that which circumstance has brought about.

Professor Spulber's account is filled with statistical and other detail, and it is perhaps rather difficult to read. The main facts of the Eastern European economic problem are set out, and it is clear from them as from other studies of the region that the solution of these problems ought to have been in some respects easier than for other underdeveloped areas. For, allowing a period of post-war rehabilitation, it can be said that the Eastern European economic situation was capable of very considerable improvement within a generation.

But Dr. Rosenstein-Rodan and others who said this were thinking in terms of international investment in the area, of a reasonable balance of heavier and lighter industry, and in some senses of planning the region as a whole. Even they might well be criticised for the largeness of their conceptions, but they were reasonable and moderate alongside the forced pace of Communist development in the area. As Professor Spulber points out, this pace has led, and is leading, to acute tensions.

A. L. MINKES.

New Ways in Management Training. By CYRIL SOFER and GEOFFREY HUTTON. Tavistock Publications Ltd. 1958. xii + 127 pp. 15s.

The most ardent supporter of management training schemes would admit that work in this field is still in an experimental stage, and that unfortunately the opportunity rarely occurs to test the success of experiments. It is also admitted that with the great expansion now taking place in the technical colleges consideration must be given to the type of management studies that should be carried out in these colleges.

In *New Ways in Management Training* Cyril Sofer and Geoffrey Hutton, of the Tavistock Institute of Human Relations, describe an investigation into the work going on in the Department of Management and Production Engineering at the Acton Technical College. It was early realised that effective development must depend on studying and interpreting the needs of local industry and of the varying types of students drawn from firms in the vicinity of the college. In order to develop this collaborative work the investigators and the staff of the department, together with representatives of the firms, undertook a survey into the training needs of a group of companies whose employees were students at the college. From this joint enterprise a number of valuable suggestions were forthcoming, some of which are apparently already leading to important modifications in the activities of the department. This is an experiment which may well have significant implications in the field of technical education.

B. N. SEEAR.

Man in Employment. By A. B. Badger. Arthur Barker Ltd. 1958. 320 pp. 25s.

The Challenge of Employee Shareholding. By George Copeman. Business Publications Ltd. 1958. 200 pp. 18s.

In writing his book Dr. Badger set out to provide a comprehensive study of modern industrial relations and if his book is measured by the range of topics touched upon he has succeeded in achieving his aim. His purpose, however, was apparently to do far more than merely give a brief descriptive account of the many different facets of the subject, since the book is subtitled *The Fundamental Principles of Industrial Relations*. Unfortunately the author rarely moves beyond a useful but rather pedestrian account of the facts, and penetrating discussion of principles is hard to find.

Take, for example, the tremendously important problem of the " closed shop ". This fundamental issue is discussed in no more than a few lines. There is no mention of the Communist problem at all, and the less admirable aspects of trade unionism receive no attention. Nor is there any bold attempt to go beyond clichés about the nature of management. The author must be as aware as anyone of the real problems that often face management—problems of personality, problems of vested interests, and problems of power—that have a profound impact on industrial relations. There is no discussion at all of the power relations between the different levels of management, or of the fears, hopes and beliefs that influence the attitude and behaviour of management. Dr. Badger never illumines a topic with an example from his own experience and one feels that as a practising personnel officer in a nationalised industry he has been inhibited by his responsibilities.

Mr. George Copeman's book suffers from no such limitations and he has revived the claim that employee-shareholding is the panacea for resolving the conflict between capital and labour. The second part of his study, which provides a summary account of ten different types of profit-sharing schemes in operation in fifty well known companies, is extremely valuable. The same cannot be said for the proselytising first part. The author is quite clearly a naive enthusiast and appears to know little or nothing about the problem he is hoping to solve by an extension of employee-shareholding. It is a pity that Mr. Copeman has done no more than repeat unsubstantiated claims in support of his faith instead of thoroughly examining the problems of industrial relations raised by employee-shareholding. Had Mr. Copeman examined the impact of employee-shareholding on industrial relations in the firms to which he refers, his book might have had more significance for the student of industrial relations.

B. C. Roberts.

SHORTER NOTICES

Prosperity and Depression. By Gottfried Haberler. Revised and Enlarged Edition. George Allen & Unwin Ltd. 1958. xviii + 520 pp. 28s.

Professor Haberler's book on the theory of cyclical movements has served successive generations of students for more than twenty years. The new edition reproduces the old Parts I and II but substitutes some recent papers for the old Part III and Appendices. The main new component is an abridged and slightly altered version of the author's paper read at the Rome congress in 1956; in it he protested against recent overstressing of "real factors" and concluded that "monetary factors comprehensively defined bear a heavy share of responsibility for short run uneconomic instability". These concise pages add notably to the current usefulness of this standard work.

Prosperity through Competition. By LUDWIG ERHARD. Thames & Hudson. 1958. xii + 260 pp. 25s.

This is in the main an account of the economic recovery of Western Germany, but although it is written by the chief architect of the post-war German economy, it adds very little to what we already know about Germany's remarkable performance. Much of the book is taken up by diatribes against the planners and by repeated eulogies of the virtues of competition, and no real attempt has been made to get beyond these empty phrases and to analyse the real cause of the rapid economic expansion. It is true that Professor Erhard has some interesting things to say about the way in which Germany's economic policy has been decided, but that does not dispel the impression that the book provides a striking example of a missed opportunity.

Hydroelectricity and Industrial Development: Quebec 1898–1940. By JOHN H. DALES. Harvard University Press. Oxford University Press. 1957. xii + 269 pp. 45s.

This book is an important contribution to the study of a relatively neglected field—electricity and economic development in the 20th century. The main theme is the story of hydroelectricity, and of the private producing companies, in Quebec. The particular study leads to the examination of many questions of general interest: the influence of hydroelectricity on the development and location of different industries; the financing and pricing policy of a capital-intensive industry such as electricity production; the role of private enterprise in both finance and promotion; the significance of hydroelectricity as an agent in the transformation of Quebec, and indeed of Canada as a whole, from an economy heavily dependent on export markets to one very much more diversified and self-reliant.

The Changing Population of the United States. By CONRAD and IRENE B. TAEUBER. John Wiley & Sons Inc., New York. Chapman & Hall Ltd. 1958. xi + 357 pp. 62s.

Being a census monograph, one of its major objectives is to provide a guide to sources of American statistics, and their pitfalls, which it admirably achieves.

Unlike other monographs, each devoted to a single topic, the Taeubers have aimed at covering a broad field. Chapters cover the growth (both in numbers and geographical spread) of the American population, migration, characteristics (education, economic activity, etc.), fertility, mortality and future prospects; and throughout, such differentials as urban/rural and sex, age and colour are a regular feature. They have thus been able to free their volume from the fog of excessive detail and have avoided being merely superficial. Those more interested in concentration on essentials may well prefer to refer to this, rather than to the other detailed volumes.

Whilst mainly a reference work, the good diagrams and lively style in this volume make the reading of substantial sections not unduly wearisome.

Population and World Politics. Edited by PHILIP M. HAUSER. The Free Press, Glencoe, Illinois. 1958. pp. 297.

This is a collection of 12 papers which were originally delivered at a Round Table of the Norman Wait Harris Memorial Foundation, held at Chicago in 1954. The book is not appropriately named; anyone hoping for a treatment discussing the relationship between demographic phenomena and the foreign or domestic policies of the principal countries will be disappointed. Eight of the papers discuss the relation between population, resources, levels of living and economic development, and, though useful summaries of present knowledge, have little that is new in them. There are four papers under the heading of population policy and politics. Kingsley Davis discusses the demographic potentials of the non-Communist world, Frank Lorimer deals with population policies and politics in Communist countries (though the gap between time of delivery and publication tends to make his paper somewhat out of date). Irene Taeuber describes the political instabilities and their relation to population in underdeveloped areas, and there is an extremely general paper by Quincy Wright on population and United States foreign policy (for which footnotes have unaccountably been omitted). The book as a whole does not add significantly to our knowledge of demography.

Development Area Policy in the North East of England. By E. ALLEN, A. J. ODBER and P. J. BOWDEN. North East Industrial and Development Association, Newcastle. 1957. 94 pp. 5s.

This is the report of an enquiry commissioned by the North East Industrial and Development Association which was undertaken by two university economists and a former research officer of the Association. The first four chapters contain a critical review of pre-war and post-war policy affecting location of industry, which gains strength from the authors' close acquaintance with the North East. Popular arguments for development area policy—such as the cyclical unemployment, social cost and diversification arguments—receive strong criticism, and the whole of the first part of the book deserves attention from those interested generally in development areas. The main part of the book deals with the North East and contains an analysis of questionnaires on the experience of new firms which were answered by 90 firms newly established in the area. The recommendations of the authors are not published; but two policies that their enquiries support are selective encouragement of firms in a limited number of growing industries, and siting near existing industrial centres rather than at isolated points of local unemployment.

Insurance. By H. A. L. COCKERELL. The English Universities Press Ltd. 1958. x + 210 pp. 6s.

This little book gives a clear and comprehensive outline of the whole range of insurance business carried on in the United Kingdom, including national insurance. While the treatment is necessarily brief, and some aspects of the subject are touched on only lightly, Mr. Cockerell's survey is balanced and accurate throughout, so that the reader who wishes to go further will have nothing to unlearn. The book can be unreservedly recommended both to members of the public who wish to learn something of the way in which the insurance industry can and does serve them and to students beginning their study of the subject.

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